

Gun Control is auth cringe

- Guns in other countries

Canada-

<https://pubmed.ncbi.nlm.nih.gov/24364129/>

Domestic violence remains a significant public health issue around the world, and policy makers continually strive to implement effective legislative frameworks to reduce lethal violence against women. This article examines whether the 1995 Firearms Act (Bill C-68) had a significant impact on female firearm homicide victimization rates in Canada. Time series of gender-disaggregated data from 1974 to 2009 were examined. Two different analytic approaches were used: the autoregressive integrated moving average (ARIMA) modelling and the Zivot-Andrews (ZA) structural breakpoint tests. There was little evidence to suggest that increased firearms legislation in Canada had a significant impact on preexisting trends in lethal firearm violence against women. **These results do not support the view that increasing firearms legislation is associated with a reduced incidence of firearm-related female domestic homicide victimization.**

<https://pubmed.ncbi.nlm.nih.gov/32555647/>

Canada implemented a series of laws regulating firearms including background and psychological screening, licensing, and training in the years 1991, 1994, and 2001. The effects of this legislation on suicide and homicide rates were examined over the years 1981 to 2016. Models were constructed using difference-in-difference analysis of firearms and non firearms death rates from 1981 to 2016. In addition, negative binomial regression was used to test for an association between rates of suicide by Canadian Province and firearms prevalence, using licensing rates as a proxy for prevalence. No associated benefit from firearms legislation on aggregate rates of male suicide was found. In men aged 45 to 59 an associated shift from firearms suicide after 1991 and 1994 to an increase in hanging resulted in overall rate ratios of 0.994 (95%CI, 0.978,1.010) and 0.993 (95%CI, 0.980,1.005) respectively. In men 60 and older a similar effect was seen after 1991, 1994, and 2001, that resulted in rate ratios of 0.989 (95%CI, 0.971,1.008), 0.994 (95%CI, 0.979,1.010), and 1.010 (95%CI, 0.998,1.022) respectively. In females a similar effect was only seen after 1991, rate ratio 0.983

(95%CI, 0.956,1.010). No beneficial association was found between legislation and female or male homicide rates. There was no association found with firearm prevalence rates per province and provincial suicide rates, but an increased association with suicide rates was found with rates of low income, increased unemployment, and the percentage of aboriginals in the population. In conclusion, firearms legislation had no associated beneficial effect on overall suicide and homicide rates. Prevalence of firearms ownership was not associated with suicide rates. Multifaceted strategies to reduce mortality associated with firearms may be required such as steps to reduce youth gang membership and violence, community-based suicide prevention programs, and outreach to groups for which access to care may be a particular issue, such as Aboriginals.

<https://pubmed.ncbi.nlm.nih.gov/22328660/>

Canada has implemented legislation covering all firearms since 1977 and presents a model to examine incremental firearms control. The effect of legislation on homicide by firearm and the subcategory, spousal homicide, is controversial and has not been well studied to date. Legislative effects on homicide and spousal homicide were analyzed using data obtained from Statistics Canada from 1974 to 2008. Three statistical methods were applied to search for any associated effects of firearms legislation. Interrupted time series regression, ARIMA, and Joinpoint analysis were performed. Neither were any significant beneficial associations between firearms legislation and homicide or spousal homicide rates found after the passage of three Acts by the Canadian Parliament--Bill C-51 (1977), C-17 (1991), and C-68 (1995)--nor were effects found after the implementation of licensing in 2001 and the registration of rifles and shotguns in 2003. After the passage of C-68, a decrease in the rate of the decline of homicide by firearm was found by interrupted regression. Joinpoint analysis also found an increasing trend in homicide by firearm rate post the enactment of the licensing portion of C-68. Other factors found to be associated with homicide rates were median age, unemployment, immigration rates, percentage of population in low-income bracket, Gini index of income equality, population per police officer, and incarceration rate. This study failed to demonstrate a beneficial association between legislation and firearm homicide rates between 1974 and 2008.

<https://pubmed.ncbi.nlm.nih.gov/18444777/>

This study presents the changes in the overall and firearm suicide rates for Québec (Canada) before and after Bill C-17, which was implemented to secure safe storage of firearms. It covers 20,009 suicide cases reported to the coroner's office. Interrupted time series analysis is used to compare suicide rates in the two periods. Firearm suicide rates have dropped among males and females, but the downward trends were not significant when compared to those prior to the law. Hanging suicide rates have risen considerably among men and women, but those

upward trends did not increase significantly when compared with those preceding the law. The decline in suicide rates involving firearms has not resulted in a parallel decline in overall suicide rates. The analyses suggest that Bill C-17 neither improved the downward trend in firearm suicide, which had already begun before the enactment of the law, nor reduced the upward trend of the overall suicide rate. Correlation analyses between firearm suicide, hanging suicide, and the overall suicide rate suggest that firearm suicide is replaced by hanging suicide among males.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=998898&rec=1&srcabs=2122854&pos=4

This paper is a timely effort to evaluate the effectiveness of the 1995 firearm legislation that created the firearm registry. In 1995, the government assumed that, by controlling the availability of firearms, the registry would reduce total criminal violence, not just gun violence, suicide and domestic abuse.

In conclusion, no convincing empirical evidence can be found that the firearm program has improved public safety. Violent crime and suicide rates remain virtually unchanged despite the nearly unlimited annual budgets during the first seven years of the firearms registry. Notwithstanding an estimated C\$ 2 billion cost to date, the firearms registry remains notably incomplete and has an error rate that remains embarrassingly high.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=1599705&rec=1&srcabs=2122854&pos=5

The evidence demonstrates that the repeal of the long-gun registry will not reduce public safety in Canada and may even improve it. After a brief review of the arguments, I examine and rebut the key points in the Coalition's letter sent to MPs on October 20, 2009.

The statistics provided here clearly shows that the long-gun registry has not been effective in reducing criminal violence and most especially that it has not saved lives. The multiple murders by shooting that have occurred since the registry was put in place prove that it is a waste of time and money.

Australia-

https://www.researchgate.net/profile/Samara_Mcphedran/publication/251255412_Gun_Laws_and_Sudden_Death_Did_the_Australian_Firearms_Legislation_of_1996_Make_a_Difference/links/566f549608ae486986b70b96/Gun-Laws-and-Sudden-D

[eath-Did-the-Australian-Firearms-Legislation-of-1996-Make-a-Difference.pdf](#)

“There is insufficient evidence to support the simple premise that reducing the stockpile of licitly held civilian firearms will result in a reduction in either firearm or overall sudden death rates.”

<https://www.cambridge.org/core/journals/the-british-journal-of-psychiatry/article/austrian-firearms-data-require-cautious-approach/FCA06C0896953B20BDC1E5CEB684214>

“Recent work demonstrates that Australia's 1996 gun laws had no significant impact on firearm homicide but that the pre-existing decline in firearm suicide accelerated post-reforms. There has been an accompanying decline in non-firearm suicides beginning in the late 1990s.”

https://www.researchgate.net/publication/241314503_The_Failed_Experiment_Gun_Control_and_Public_Safety_in_Canada_Australia_England_and_Wales

“The divergence between Australia and the United States is even more apparent with violent crime. While violent crime is decreasing in the United States, it is increasing in Australia. Over the past six years, the overall rate of violent crime in Australia has continued to increase. Robbery and armed robbery rates continue to rise. Armed robbery has increased 166% nationwide. The confiscation and destruction of legally owned firearms cost Australian taxpayers at least \$500 million. The costs of the police services bureaucracy, including the hugely costly infrastructure of the gun registration system, has increased by \$200 million since 1997. And for what? There has been no visible impact on violent crime. It is impossible to justify such a massive amount of the taxpayers’ money for no decrease in crime. For that kind of tax money, the police could have had more patrol cars, shorter shifts, or maybe even better equipment. Think of how many lives might have been saved.”

<https://www.jpands.org/hacienda/comm8.html>

Two years after the ban, there have been further increases in crime: armed robberies by 73 percent; unarmed robberies by 28 percent; kidnappings by 38 percent; assaults by 17 percent; manslaughter by 29 percent, according to the Australian Bureau of Statistics.

<https://jamanetwork.com/journals/jama/article-abstract/2530362>

Following the enactment of gun law reforms in Australia in 1996, there were no mass firearm killings through May 2016. There was a more rapid decline in firearm deaths between 1997 and 2013 compared with before 1997, but also a decline in total nonfirearm suicide and homicide deaths of a greater magnitude. Because of this, it is not possible to determine whether the change in firearm deaths can be attributed to the gun law reforms.

<https://link.springer.com/article/10.1007/s00127-008-0435-9>

The implemented restrictions may not be responsible for the observed reductions in firearms suicide. Data suggest that a change in social and cultural attitudes could have contributed to the shift in method preference.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3086324

In 1996 Australia implemented arguably the most ambitious gun control effort ever attempted, banning all semiauto rifles and shotguns and all pump-action rifles and shotguns, and buying the banned guns already in circulation. Chapman, Alpers, and Jones (2016) produced what is arguably the most extensive evaluation, concluding that the measure was a success. In fact, their own data indicated that the effort failed to reduce homicides, suicides, or unintentional firearms deaths. It is even questionable whether the effort reduced mass shootings, the problem that had triggered the gun control effort in the first place.

http://www.cjcj.org/uploads/cjcj/documents/mass_shootings.pdf

The development of legislation aimed at reducing the incidence of firearm-related death is an ongoing interest within the spheres of criminology, public policy, and criminal justice. Although a body of research has examined the impacts of significant epochs of regulatory reform upon firearm-related suicides and homicides in countries like Australia, where strict nationwide firearms regulations were introduced in 1996, relatively little research has considered the occurrence of a specific type of homicide: mass shooting events. The current paper examines the incidence of mass shootings in Australia and New Zealand (a country that is socioeconomically similar to Australia, but with a different approach to firearms regulation) over a 30 year period. It does not find support for the hypothesis that Australia's prohibition of certain types of firearms has prevented mass shootings, with New Zealand not experiencing a mass shooting since 1997 despite the availability in that country of firearms banned in Australia. These findings are discussed in the context of social and economic trends.

<https://www.sciencedirect.com/science/article/abs/pii/S1359178916300258>

Developing legislative interventions to address firearm misuse is an issue of considerable public policy interest across many countries. However, systematic reviews of evidence about the efficacy of legislative change in reducing lethal firearm violence have only considered research examining the United States of America, a country that is unique among developed nations in its approach to firearm ownership. To inform international policy development, there is a need to consider other countries' experiences with gun law amendments. The current study used systematic literature search methods to identify evaluation-focused studies examining the impacts of legislative reform on firearm homicide in Australia, a country that made significant changes to its gun laws in the mid-1990s. Five studies met the inclusion criteria. These examined various different time periods, and used a range of different statistical analysis methods. No study found statistical evidence of any significant impact of the legislative changes on firearm homicide rates. The strengths and limitations of each study are discussed. Findings from this review provide insights into strategies and policies that may, and may not, be effective for reducing lethal firearm violence.

Australian studies have not found evidence of changes in lethal violence following gun law reform.

Empirical findings about Australian gun law reform contradict 'popular' views about those laws.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=1540791&rec=1&srcabs=2122854&pos=2

The 1996-1997 National Firearms Agreement (NFA) in Australia introduced strict gun laws, primarily as a reaction to the mass shooting in Port Arthur, Tasmania, in 1996, where 35 people were killed. Despite the fact that several researchers using the same data have examined the impact of the NFA on firearm deaths, a consensus does not appear to have been reached. In this paper, we reanalyze the same data on firearm deaths used in previous research, using tests for unknown structural breaks as a means to identifying impacts of the NFA. **The results of these tests suggest that the NFA did not have any large effects on reducing firearm homicide or suicide rates.**

<https://pubmed.ncbi.nlm.nih.gov/30252523/>

The NFA had no statistically observable additional impact on suicide or assault mortality attributable to firearms in Australia.

U.K and Wales

https://www.researchgate.net/publication/241314503_The_Failed_Experiment_Gun_Control_and_Public_Safety_in_Canada_Australia_England_and_Wales

In the past 20 years, both Conservative and Labour governments have introduced restrictive firearm laws; even banning all handguns in 1997. Unfortunately, these Draconian firearm regulations have totally failed. The public is not any safer and may be less safe. Police statistics show that England and Wales are enduring a serious crime wave. In contrast to handgun-dense United States, where the homicide rate has been falling for over 20 years, the homicide rate in handgun-banning England and Wales has been growing. In the 1990s alone, the homicide rate jumped 50%, going from 10 per million in 1990 to 15 per million in 2000. Police statistics show that violent crime in general has increased since the late 1980s and, in fact, since 1996 has been more serious than in the United States. The firearm laws may even have increased criminal violence by disarming the general public. Despite Britain's banning and confiscating all handguns, violent crime, and firearm crime, continue to grow.

The Firearms (Amendment) Act of 1988 was brought in by the Conservative government following the Hungerford incident and the Firearms (Amendment) Act of 1997, which banned all handguns, was introduced by the Labour government following the shooting in Dunblane in 1996 (Greenwood 2001; Munday and Stevenson 1996). Unfortunately, these Draconian firearm regulations have not curbed crime (see Malcolm 2002). Police statistics show that England and Wales are enduring a serious crime wave. In contrast to North America, where the homicide rate has been falling for over 20 years, the homicide rate in England and Wales has been growing over the same time period. In the 1990s alone, the homicide rate jumped 50%, going from 10 per million in 1990 to 15 per million in 2000 (Home Office 2001).

Police statistics show that violent crime in general has increased since the late 1980s and, in fact, since 1996 has been more serious than in the United States (figure 2). The rate of violent crime has jumped from 400 per 100,000 in 1988 to almost 1,400 per 100,000 in 2000. (An unknown amount of the recent increase may be attributed to changes in the recording rules in 1998 and 1999.) In contrast, not only are violent crime rates lower in the United States, they are continuing to decline

Property crime has also grown more serious since the early 1980s. Although

property crime rates have fallen back somewhat in the 1990s, they are still higher in 1997, at over 8000 per 100,000 population, than they had been in 1982, at about 6,000 per 100,000) (figure 3). In contrast, property crime rates are falling in the United States (Home Office 2001; Federal Bureau of Investigation, 2003).

Clearly, there is no evidence that firearm laws have caused violent crime to fall. The firearm laws may even have increased criminal violence by disarming the general public. Despite banning and confiscating all handguns, violent crime—and firearm crime—continue to grow. The number of violent crimes involving Handguns has increased from 2,600 in 1997/1998 to 3,600 in 1999/2000. Firearm crime has increased 200% in the past decade.

<http://www.sfu.ca/~mauser/papers/letters/DrUSoped104.pdf>

Abstract: Should the US copy British gun laws? Politicians claim that restrictive firearm regulations will make society safer, but in a recent report published by the Fraser Institute in Vancouver, BC, in Canada, I found that restrictions on gun ownership did not reduce either homicide rates or the rate of violent crime in any of the countries I examined. If the goal truly is to improve public safety, governments are urged to seek more effective approaches.

<https://publications.parliament.uk/pa/cm200203/cmselect/cmniaf/67/2071702.htm>

There is nothing in the statistics for England and Wales to suggest that either the stricter controls on handguns prior to 1997 or the ban imposed since have controlled access to such firearms by criminals. Nor is there anything to show that the apparently more lax controls on shotguns has caused criminals to favour them against the supposedly much more difficult to acquire pistol.

The evidence shows that there would be no benefit to public safety in extending the ban on handguns to Northern Ireland and that controls on shotguns could be made simpler, more flexible and less bureaucratic without increasing the risk to public safety.

New Zealand-

<https://pubmed.ncbi.nlm.nih.gov/16476153/>

After legislation, the mean annual rate of firearm-related suicides decreased by 46% for the total population ($p < 0.0001$), 66% for youth (15-24 years; $p < 0.0001$) and 39% for adults (≥ 25 years; $p < 0.01$). The fraction of all suicides accounted for by firearm-related suicides also reduced for all three populations ($p < 0.0001$). However, the introduction of firearms legislation was not associated with reductions in overall

rates of suicide for all three populations.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2122854

The development of legislation aimed at reducing the incidence of firearm-related death is an ongoing interest within the spheres of criminology, public policy, and criminal justice. Although a body of research has examined the impacts of significant epochs of regulatory reform upon firearm-related suicides and homicides in countries like Australia, where strict nationwide firearms regulations were introduced in 1996, relatively little research has considered the occurrence of a specific type of homicide: mass shooting events. The current paper examines the incidence of mass shootings in Australia and New Zealand (a country that is socioeconomically similar to Australia, but with a different approach to firearms regulation) over a 30 year period. It does not find support for the hypothesis that Australia's prohibition of certain types of firearms has prevented mass shootings, with New Zealand not experiencing a mass shooting since 1997 despite the availability in that country of firearms banned in Australia. These findings are discussed in the context of social and economic trends.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3045830

The Arms Legislation Bill of 2019 creates a regulatory behemoth that focuses exclusively on non-violent, lawful firearms ownership. No convincing evidence has been presented to justify the numerous restrictions on the lawful use and possession of arms and ammunition.

A review of the international evidence shows that civilian firearms ownership does not pose a public safety hazard. Moreover, there is no convincing evidence that the introduction of strict regulations on firearms ownership acts to reduce criminal violence or suicide rates.

The Arms Legislation Bill of 2019 focuses exclusively on firearms, but terrorists use a variety of methods to murder large numbers of people. How will the billions of dollars spent on creating an ineffective if massive police bureaucracy be justified if another mass murder occurs again whether or not firearms are involved?

Concealed Carry

<https://www.tandfonline.com/doi/abs/10.1080/13504851.2013.854294>

Mark Gius, 2013

These results suggest that restrictive concealed weapons laws may cause an increase in gun-related murders at the state level. The results of this study are consistent with some prior research in this area, most notably Lott and Mustard (1997).

<https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1465-7295.1998.tb01711.x>

Bartley, Cohen, 1998

Lott and Mustard [1997] provide evidence that enactment of concealed handgun ("right-to-carry") laws deters violent crime and induces substitution into property crime. A critique by Black and Nagin [1998] questions the particular model specification used in the empirical analysis. In this paper, we estimate the "model uncertainty" surrounding the model specified by Lott and Mustard using an extreme bound analysis (Leamer [1983]). We find that the deterrence results are robust enough to make them difficult to dismiss as unfounded, particularly those findings about the change in violent crime trends. The substitution effects are not robust with respect to different model specifications. (JEL K42)

https://econpapers.repec.org/article/ucpjlstud/v_3a27_3ay_3a1998_3ai_3a1_3ap_3a221-43.htm

Lott, 1998

Dan A. Black and Daniel S. Nagin state that my article with David Mustard assumes that the effect of concealed-handgun laws is constant over time, that the effect is the same across states, that the article does not control for local time trends, and that we did not investigate whether the results were sensitive to the missing values of the arrest rate. None of these claims are correct, and this is easily verified by anyone who reads the original article. Their statement that the results are sensitive to including Florida applies to fewer than 1 percent of the regressions that I have reported. Using results from previous drafts of Black and Nagin's comment as well as new estimates of my own, I provide additional evidence that allowing law-abiding citizens to carry concealed handguns deters criminals. Violent crime rates were rising before the law was passed and fell thereafter.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=162978

Bartley, 1999

This article presents one possible alternative to rationing or banning the sale of guns to reduce crime. It offers that enacting "right-to-carry" laws to allow the carrying of concealed handguns can accomplish this task. Chaudhri and Geanakoplos (1998) in Economics Letters hypothesize that a reduction in the supply of guns will increase their cost, subsequently curtailing the quantity of guns demanded and used, specifically for the commission of crimes. I believe that this issue with guns and crime is not so obvious. The demand and supply of

crime involve a price variable, which is the average net payoff to the potential offender, after accounting for the probabilities and costs of arrest and of conviction, according to Cook (1986) in *Crime and Justice: An Annual Review of Research*. One way to reduce crime is to decrease the demand for it by increasing the cost of committing crime. Higher prices for guns, which are potential instruments of crime, could be an answer. I propose a more realistic solution. I believe it will be difficult to ban the sale of new guns, much less to reduce the large stock of guns already existing, to increase their price. By enacting these concealment laws, which already exist in thirty-one states, potential offenders may now face more victims that are armed. Adding the externality effect that the offenders will not know who is armed, the increased chance of bodily injury or death to offenders should substantially raise their perceived costs, thus lowering their average net payoff and so crimes committed. Some research has substantiated these claims, including Bartley and Cohen (1998) in *Economic Inquiry*. The passage of other laws can have this same effect, but this article was intended to show that rationing guns is not necessarily the best method of reducing crime.

<http://bingweb.binghamton.edu/~fplass/gun.pdf>

Plassmann, Tideman, 2001

We find that the effects of such laws vary across crime categories, U.S. states, and time, and that such laws appear to have statistically significant deterrent effects on the numbers of reported murders, rapes, and robberies.

https://www.researchgate.net/publication/4980809_Criminal_Deterrence_Geographic_Spillovers_and_Right-to-Carry_Concealed_Handguns

Bronars, Lott, 1998

“Taken together these results imply that concealed handguns deter criminals and that the largest reductions in violent crime will be obtained when all the states adopt these laws.”

<https://www.journals.uchicago.edu/doi/abs/10.1086/323313>

Moody, 2001

In 1997, John Lott and David Mustard published an important paper in which they found that right-to-carry concealed weapons laws reduce violent crime. Although Lott and Mustard appear to do all possible variations of the analysis, a closer reading reveals that the study might suffer from several possibly important errors. I reestimate the model and check for incorrect functional form, omitted variables, and possible second-order bias in the *t*-ratios. Lott and Mustard's

basic conclusions are generally robust with respect to these potential econometric problems. Overall, right-to-carry concealed weapons laws tend to reduce violent crime. The effect on property crime is more uncertain. I find evidence that these laws also reduce burglary.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=286081

Benson, Mast, 2001

In this study, we use county data on private security establishments and employment for 1977-92 to test two hypotheses. First, we test whether private security deters crime. Second, we test whether Lott and Mustard's estimates of the impact of shall-issue laws on crime are biased due to lack of controls for private security. We find little evidence that private security is reducing the crime rates for assault or larceny. Some estimates suggest murder, robbery, and/or auto theft may be deterred by private security, although these results are not robust. Of all the index crime categories, only rape is estimated to have a consistent negative relationship with private security. In addition, we find little evidence that the Lott and Mustard results are biased due to lack of controls for the private security measures employed in this study.

<https://www.journals.uchicago.edu/doi/abs/10.1086/338345>

Olson, Maltz

Recently, a number of states have enacted laws that allow citizens to carry concealed weapons. This “natural experiment” was analyzed by John Lott and David Mustard, who found that these right-to-carry laws reduced violent crime, with a substitution toward property crimes, in those jurisdictions that adopted this law. Of particular importance, they found that homicide was reduced significantly, with even greater declines in larger jurisdictions. Their findings came at the same time that major reductions in homicide were occurring in many cities and states that did not change their gun-carrying policies, which lead to questions of whether their finding was spurious, caused by problems with the data or methods. In this paper, we describe an analysis that looks at the effect of changing one aspect of their homicide analysis: disaggregating homicide data by weapon type, victim characteristics, and victim-offender relationships. The results show that the liberalized carrying laws are associated with a number of effects, some that are consistent with those found by Lott and Mustard and others that are not. It also illustrates the importance of being able to look beyond aggregate crime measures in this type of examination, which is currently possible on a national level only for the crime of homicide.

<https://link.springer.com/article/10.1023/A:1023054204615>

Lott, Whitley

Maltz and Targonski (2002) have provided an important service by disaggregating the county level data to help researchers examine measurement errors in the county level data, but their conclusion that county-level crime data, as they are currently constituted, should not be used, especially in policy studies is not justified. All data has measurement error, presumably even their measures of this error. Unfortunately, however, Maltz and Targonski provide no systematic test for how bad the data are. Their graphs obscure both the small number of counties affected, that these are rural counties, and that just because some of the population in a county is not represented in calculating the crime rate, that is not the same thing as showing that the reported number is in error. Nor do they provide evidence for the more important issue of whether there is a systematic bias in the data. The evidence provided here indicates right-to-carry laws continue to produce substantial reductions in violent crime rates when states with the greatest measurement error are excluded. In fact, restricting the sample results in somewhat larger reductions in murders and robberies, but smaller reductions in aggravated assaults.

<https://www.degruyter.com/view/journals/bejeap/4/1/article-bejeap.2004.4.1.1182.xml>

Helland, Tabarrok, 2004

We reexamine Mustard and Lott's controversial study on the effect of "shall-issue" gun laws on crime using an empirical standard error function randomly generated from "placebo" laws. We find that the effect of shall-issue laws on crime is much less well-estimated than the Mustard and Lott (1997) and Lott (2000) results suggest. We also find, however, that the cross equation restrictions implied by the Lott-Mustard theory are supported. A boomlet has occurred in recent years in the use of quasi-natural experiments to answer important questions of public policy. The intuitive power of this approach, however, has sometimes diverted attention from the statistical assumptions that must be made, particularly regarding standard errors. Failing to take into account serial correlation and grouped data can dramatically reduce standard errors suggesting greater certainty in effects than is actually the case. We find that the placebo law technique (Bertrand, Duflo and Mullainathan 2002) is a useful addition to the econometrician's toolkit.

<https://www.nap.edu/read/10881/chapter/13>

James Q. Wilson, 2005

In sum, I find that the evidence presented by Lott and his supporters suggests that RTC laws do in fact help drive down the murder rate, though their effect on other crimes is ambiguous.

<https://econjwatch.org/articles/the-debate-on-shall-issue-laws>

Moody, Marvell, 2008

“Shall-issue” laws require authorities to issue concealed-weapons permits to anyone who applies, unless the applicant has a criminal record or a history of mental illness. A large number of studies indicate that shall-issue laws reduce crime. Only one study, an influential paper in the Stanford Law Review (2003) by Ian Ayres and John J. Donohue III, implies that these laws lead to an increase in crime. We apply an improved version of the Ayres and Donohue method to a more extensive data set. Our analysis, as well as Ayres and Donohue’s when projected beyond a five-year span, indicates that shall-issue laws decrease crime and the costs of crime. Purists in statistical analysis object with some cause to some of methods employed both by Ayres and Donohue and by us. But our paper upgrades Ayres and Donohue, so, until the next study comes along, our paper should neutralize Ayres and Donohue’s “more guns, more crime” conclusion.

https://www.researchgate.net/publication/4980809_Criminal_Deterrence_Geographic_Spillovers_and_Right-to-Carry_Concealed_Handguns

Lott, Bronars 1998

Increased law enforcement or penalties may deter crime, but they may also cause criminals to move to other crimes or other areas. This paper examines whether the adopting a shall issue concealed weapons law in one state alters crime in neighboring areas. The benefits that a county obtains from its state passing a shall issue concealed handgun law are generally stronger than those found in previous work. Spillover effects on neighboring areas are almost always deleterious. Criminals tend to move across communities more readily in response to changes in concealed handgun laws than in response to changes in arrest rates. The spillover effects are surprisingly large, especially for property crimes, thus questioning existing research which ignores these considerations. The spillovers are immediate and increase over time (with the exception of assaults and auto theft). Except for rapes, the negative effects of a neighbor's law are mitigated by having one's own state adopting the law. Taken together these results imply that concealed handguns deter criminals and that the largest reductions in violent crime will be obtained when all the states adopt these laws. We find little evidence that increased arrest rates create similar spillovers.

<https://pdfs.semanticscholar.org/807f/88ecb7aaad734bd07a6f5879570eb9d46d91.pdf>

Plassmann & Whitley, 55 STAN. L. REV. 1313 (2003).

Analyzing county-level data for the entire United States from 1977 to 2000, we find annual reductions in murder rates between 1.5% and 2.3% for each additional year that a right-to-carry law is in effect. For the first five years that such a law is

in effect, the total benefit from reduced crimes usually ranges between approximately \$2 billion and \$3 billion per year. Ayres and Donohue have simply misread their own results. Their own most general specification that breaks down the impact of the law on a year-by-year basis shows large crime-reducing benefits. Virtually none of their claims that their county-level hybrid model implies initial significant increases in crime are correct. Overall, the vast majority of their estimates based on data up to 1997 actually demonstrate that right-to-carry laws produce substantial crime-reducing benefits. We show that their models also do an extremely poor job of predicting the changes in crime rates after 1997.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=161637

Lott, Landes, 1999

Few events obtain the same instant worldwide news coverage as multiple victim public shootings. These crimes allow us to study the alternative methods used to kill a large number of people (e.g., shootings versus bombings), marginal deterrence and the severity of the crime, substitutability of penalties, private versus public methods of deterrence and incapacitation, and whether attacks produce copycats. Yet, economists have not studied this phenomenon. Our results are surprising and dramatic. While arrest or conviction rates and the death penalty reduce normal murder rates, our results find that the only policy factor to influence multiple victim public shootings is the passage of concealed handgun laws. We explain why public shootings are more sensitive than other violent crimes to concealed handguns, why the laws reduce both the number of shootings as well as their severity, and why other penalties like executions have differential deterrent effects depending upon the type of murder.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2525625

We direct the reader to a more extensive discussion of these matters (Moody et al. 2012). In that paper we replicate the NRC tables using the same data set that Lott sent to the NRC and over a hundred researchers around the world, including one of the authors (Moody), who still has the original data set on a server. We also use the correct model specification to replicate the NRC tables using the data set that ADZ collected. We find that the results are essentially the same as the estimates based on the Lott data set.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2866445

Charles D. Phillips, Texas A&M University School of Public Health, published a paper in 2015 entitled “**Concealed** Handgun Licensing and Crime in Four States.” He and his research team concluded that **concealed** handgun licensing had no

beneficial effect on crime, and that the main driving force behind more people obtaining a license was the presence of federally licensed firearms dealers. However, there are a number of errors, assumptions, and miscalculations in his research that justify revisiting the question of the relationship between **concealed carry** laws and crime.

<https://www.sciencedirect.com/science/article/abs/pii/S107275151832074X>

During the study period, all states moved to adopt some form of concealed-carry legislation, with a trend toward less restrictive legislation. After adjusting for state and year, there was no significant association between shifts from restrictive to nonrestrictive carry legislation on violent crime and public health indicators. Adjusting further for poverty and unemployment did not significantly influence the results.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2866445

In order to “prove” that concealed carry licensing had no effect on crime, the authors had to create a small, non-representative dataset. They also used non-standard and inconsistent criteria to manipulate their dataset into arriving at their conclusions. It’s also significant that the authors, and the peer reviewers for the professional publication, ignored or missed numerous factual errors in the data. It should be no surprise that an examination of a larger – and more reliable – dataset shows that the growth of liberalized, shall-issue concealed carry laws had a beneficial impact on violent crime

Red Flag Laws

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3316573

Red flag laws had no significant effect on murder, suicide, the number of people killed in mass public shootings, robbery, aggravated assault, or burglary. There is

some evidence that rape rates rise. These laws apparently do not save lives.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3492120

Seventeen states have passed Red Flag or Extreme Risk Protection Order (ERPO) laws which allow police, family members, individuals living in the same residence, and others to file a petition for a court order temporarily seizing the firearms of persons accused to be a danger to themselves or others. The theory is that some individuals could pose a danger to themselves and others that could be made worse by the presence of firearms. Therefore, a policy that denied the individual access to their firearms, if only temporarily, might indeed save lives. However, it is possible that these laws could increase homicide or suicide. If a troubled person is aware of the existence of a Red Flag law, he or she may well not seek help because of the threat of an ERPO. Also, the enforcement of the orders could also have perverse consequences.

Two states have considerable experience with ERPO's: Connecticut, since 1999, and Indiana since 2005. We use synthetic controls and difference in differences methods to evaluate these laws. The experience in both Connecticut and Indiana is that red flag laws have had no significant effect on either homicide or suicide. We also find that ERPO laws have had no significant effect on deaths or injuries from mass public shootings.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3653555

In Connecticut, confiscation orders may be issued ex parte. Later, the respondent will have an opportunity to tell his or her side of the story in court. In Connecticut, once a judge eventually hears the respondent's side of the story, 32 percent of confiscation orders are overturned. A study in Marion County, Indiana reported similar results.

As will be detailed below, Connecticut's 32 percent reported error rate is likely an underestimate, since government officials pressure respondents not to retain counsel and contest orders.

Gun Licensing

<https://crimeresearch.org/2015/02/what-happened-to-violent-crime-in-massachusetts-after-the-the-1998-firearms-licensing-law/>

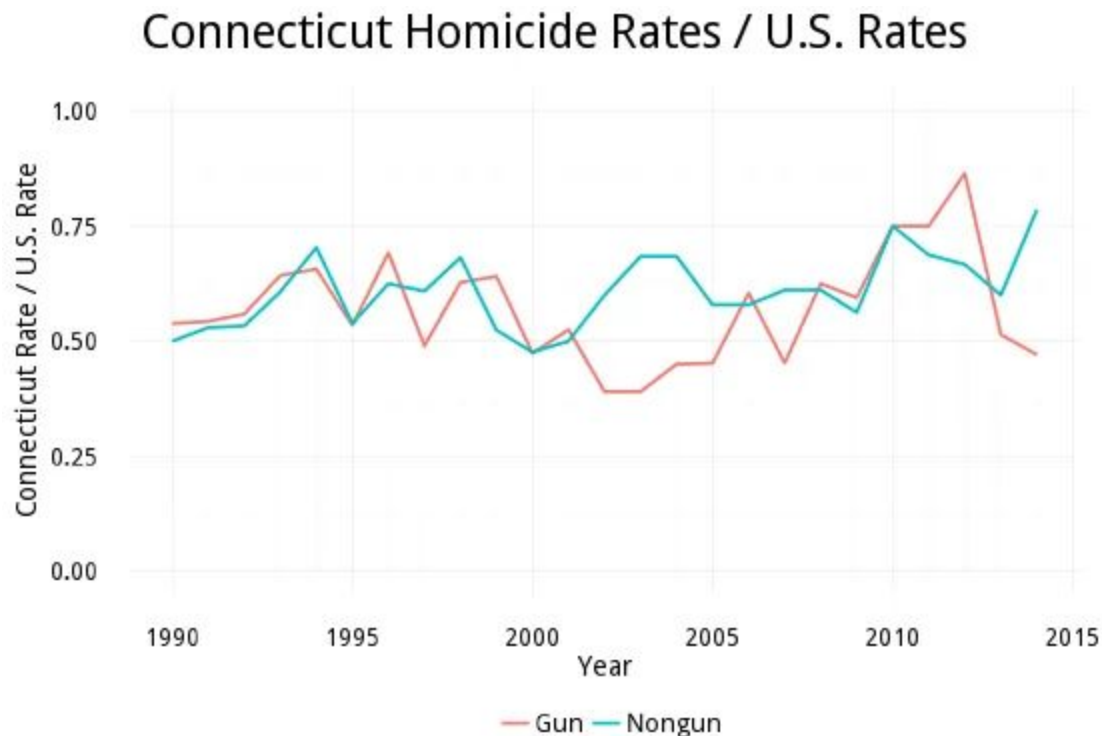
<https://www.boston.com/news/local-news/2013/02/03/gun-crimes-increase-i>

[n-massachusetts-despite-tough-gun-laws](#)

In 2011, Massachusetts recorded 122 murders committed with firearms, a striking increase from the 65 in 1998, said Fox, the Northeastern professor. Nationwide, such murders increased only 3 percent from 1999 to 2010, the CDC says.

There were increases in other crimes involving guns in Massachusetts, too. From 1998 to 2011, aggravated assaults with guns rose 26.7 percent. Robberies with firearms increased 20.7 percent during that period, according to an FBI analysis conducted for the Globe. . . .

<https://www.nationalreview.com/corner/connecticuts-1995-gun-permit-law-s-till-did-not-reduce-firearm-homicides-by-40-percent/>



<https://crimeresearch.org/2015/06/daniel-websters-cherry-picked-claim-that-firearm-homicides-in-connecticut-fell-40-because-of-a-gun-licensing-law/>

Their results are also extremely sensitive to the last year that they pick. While it is true that Connecticut's firearm homicide rate fell by 40% from 1995 to 2005, it only fell by 16% between 1995 and 2006 and 12.5% between 1995 and 2010. Meanwhile the drops for the US and the rest of the Northeast are much greater.

From 1995 and 2006, the firearm homicide rates for the US and the rest of the Northeast fell respectively by 27% and 22%. From 1995 and 2010, the drops were 39% and 31%. The longer samples show a relative increase in Connecticut's firearm homicide rate whether Rudolph et al. had looked at one additional year or five additional years.

The authors say that they limit the data to 2005 because one paper that they cite looked at only 10 years after a law that they were investigating (p. 4: **"We conclude the post-law period in 2005 to limit extrapolation in our predictions of the counterfactual to 10 years, as has been done previously"**). But just because a study on cigarette smoking looks at 12 years (not 10 as claimed (**Proposition 99 went into effect on January 1, 1989** and their sample went until 2000)) after the law was in effect, doesn't explain why a study on crime would do the same thing. Indeed, the reason given by the authors that Rudolph et al. cite isn't applicable to the current paper (p. 16: **"It ends in 2000 because at about this time anti-tobacco measures were implemented across many states, invalidating them as potential control units"**). There was no similar adoption across the states of handgun licensing laws. Yet, if Rudolph et al. had gone for this 12th year as the study that they cite does, it would have dramatically altered their results.

In three of the four years immediately after the law was passed in 1995, Connecticut's firearm homicide rate rose relative to the firearm homicides in Northeastern States. But there is no theory offered for why Connecticut's firearm homicide rate would first rise relative to other Northeastern states, then fall relative to them for six years, and then rise relative to them for four of the next five years.

<https://crimeresearch.org/2015/06/cprc-at-fox-news-connecticuts-strict-gun-licensing-law-linked-to-steep-drop-in-homicides-not-really/>

The study cherry picks which states with gun licensing laws are examined, which years are looked at, and the type of crime to study. Any normal researcher would look at *all* the states in the country that have passed a similar law and compares the changes in crime trends between those states that passed the laws to those that didn't.

Sure, from 1995 to 2005 the firearm homicide rate in Connecticut did indeed fall from 3.13 to 1.88 per 100,000 people, a 40% drop over a ten-year period. Not mentioned is that the firearms homicide rate was falling even faster immediately before the licensing law went into effect, falling from 4.5 to 3.13 per 100,000 residents — more than a 30 percent drop in just two years.

When researchers throw out data, there had better be a good reason. They didn't have one. They cite a paper that looked at the impact of smoking for 12 years after cigarette taxes were increased. What cigarettes have to do with explaining crime rates and what 12 years has to do with only looking at 10 years of data is never explained, though possibly they thought no one would actually read the paper they cited.

In any case, their results change appreciably if just one more year is added to their data. Between 1995 and 2006, Connecticut's firearm homicide rate fell by just 16 percent. By comparison, the rates for the U.S. and the rest of the Northeast fell respectively by 27 percent and 22 percent. If Connecticut's firearm homicide rate didn't fall as much as the rest of the country, why should we think that the licensing law was so beneficial?

Why the authors of the study chose to ignore other violent crimes also becomes clear pretty quickly. Relative to the rest of the United States, Connecticut's overall violent crime rate as well as its robbery and aggravated assault rates were clearly falling prior to the prior to the 1995 law and rising afterwards. Rape was unchanged. . . .

<https://crimeresearch.org/2014/02/cprc-at-fox-news-media-cherry-picks-missouri-gun-data-to-make-misleading-case-for-more-control/>

While it is true that the murder rate in Missouri rose 17 percent relative to the rest of the U.S. in the five years after 2007, it had actually increased by 32 percent during the previous five years. The question is why the Missouri murder rate was increasing relative to the rest of the United States at a slower rate after the change in the law than it did prior to it. Missouri was on an ominous path before the law was ended.

Simply looking at whether murder rates were higher after the law was rescinded than before misses much of what was going on. Most likely, getting rid of the law slowed the growth rate in murders.

<https://crimeresearch.org/2014/02/what-does-missouri-show-about-the-benefits-from-universal-background-checks-the-forthcoming-journal-of-urban-health-study-by-the-bloomberg-school-of-public-health/>

There are a lot of problems with how the tests here are conducted, particularly cherry picking one state when many states have these laws, but even if one accepts the way all those decisions were made, the change in murder rates don't change what people think that they do. The results are more complicated than simply looking at what the average murder rate before and after rescinding the Missouri law. While it is true that the murder rates in Missouri rose 17 percent relative to the rest of the US after the law was changed, it had actually increased by 32 percent during the five years prior to the change. The question is why the Missouri murder rate was increasing relative to the rest of the US at a slower rate after the change in the law than it did prior to it. The period 1999 to 2012 is picked because that is the period of time examined in Webster's study.

Other questions are why the paper only examines murder rates and not any other type of violent crime or why only murder rates from 1999 and later are examined. Again, the answer is clear: none of the other violent crime rates, including robbery, showed the change that Webster desired. As to the years studied, one would think that since Missouri's law went into effect in 1981 researchers would study the impact on crime both when the law was adopted and when it was repealed. Yet, again, the answer for why this wasn't done is obvious.

Background Checks (CBC and regular)

<https://crimeresearch.org/2017/11/nydythe-federal-background-check-system-mess-why-wont-democrats-gun-control-advocates-press-fix/>

<https://www.nydailynews.com/opinion/federal-background-check-system-mess-article-1.3617217>

Within hours of the slaughter of 26 people at the church in Sutherland Springs, Texas, Democrats were calling for expanded background checks to cover the private transfers of guns. If they had waited a few hours, they

would have learned that such a law wouldn't have stopped the attack in Texas. Nor would it have stopped any of the other mass public shootings since at least 2000.

In 2010, the last year the Bureau of Justice Statistics released a full annual report on the operation NICS system (the Obama administration stopped releasing annual reports after that), there were 72,659 denials, but only 44 federal prosecutions and just 13 convictions.

Of those 13 convictions, only six were for possession of a firearm by a felon. There were an equally small number of state prosecutions. The reason for the big gap between the 72,659 and the 44 federal prosecutions is that these aren't real cases. It is one thing to stop a criminal from buying a gun. It is something entirely different to stop someone because they have a name similar to a criminal.

Over time these "false positives" have added up to several million people. The reason for these mistakes is simple: using roughly phonetically similar names and birthdates just doesn't allow for much accuracy.

It is something that is easy to fix, just by requiring that the government use all the information the government collects when gun buyers fill out the 4473 form (e.g., their Social Security numbers and addresses). . . .

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2711323

Persistent claims have been made that expanding background checks to include any private transfers of guns would reduce mass public shootings. Yet, this is the first study to systematically look to see if that is true. In fact there is no evidence that these laws reduce the risk of these attacks. Examining all 47 mass public shootings in the US from 2000 through 2015, we find that states adopting additional background checks on private transfers see a statistically significant increase in the rate of killings (80% higher) and injuries (101%) from mass public shootings. There is not one mass public shooting that occurred over that period that these checks would have been prevented.

<https://pubmed.ncbi.nlm.nih.gov/29613872/>

We found no evidence of an association between the repeal of comprehensive

background check policies and firearm homicide and suicide rates in Indiana and Tennessee. In order to understand whether comprehensive background check policies reduce firearm deaths in the United States generally, more evidence on the impact of such policies from other states is needed.

<https://www.sciencedirect.com/science/article/abs/pii/S1047279718306161>

CBC and MVP policies were not associated with changes in firearm suicide or homicide. Incomplete and missing records for background checks, incomplete compliance and enforcement, and narrowly constructed prohibitions may be among the reasons for these null findings.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3607100

State laws mandating background checks require that persons seeking to acquire a firearms undergo a background check for a record of criminal convictions and for other statuses at high risk of future violence. A few states have enacted laws extending these checks to cover gun transfers among private persons, not just those involving licensed gun dealers. That is, they provide for so-called “universal background checks” (UBCs). The same kind of law has been proposed at the federal level. The effectiveness of UBCs is dependent on how many people seeking to acquire a gun from a private party comply with the required background check. Colorado and Oregon provide publicly available data on the numbers of background checks, data that distinguish checks on attempted private transfers from checks on dealer transfers. Combined with estimates of total private gun acquisitions (with or without checks), these data indicate that only 10.6% of private transfers in Colorado in 2019 and 3.5% of those in Oregon in 2017 were subjected to a state-mandated background check. Compliance among those trying to get a gun via a private transfer appears to be low, which should temper expectations for the impact of UBCs on firearms acquisition by prohibited persons.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2249317

What is the statistical evidence of the effects of state mandatory firearms **background** check laws on murder rates? For states that adopted or repealed such laws after 1960 (when consistent and high quality murder rate data becomes available), an interrupted time series analysis shows that two out of eight states have statistically significant changes in murder rates. Of the six states with statistically insignificant changes in murder rates, four had higher murder rates during the **background** check period. This suggests that mandatory **background** check laws are at best unproven at reducing murder rates.

<https://www.rand.org/research/gun-policy/analysis/background-checks/violent-crime.html>

<https://crimeresearch.org/2014/12/cprc-in-the-associated-press-on-background-checks/>

Take the numbers for 2009. There were 71,010 initial denials. Of those, only 4,681, or 6.6 percent, were referred to the BATF field offices for further investigation. As a report on these denials by the U.S. Department of Justice indicates, “The remaining denials (66,329 – 93%) did not meet referral guidelines or were overturned after review by Brady Operations or after the FBI received additional information.” They are making sure that the reason that the person has been flagged is for a reason that could prohibit them from getting a gun, that they have the right person, and that the reason that they have been flagged is actually correct. To put it differently, the initial review didn’t find that these individuals had a record that prevented them from buying a gun. (Numbers for 2010 are very similar and are **available here**. The Obama administration has stopped releasing this data after 2010.)

Still that isn’t the end of the story. Of these 4,681 referrals, over 51 percent, or 2,390 cases, involve “delayed denials,” cases where a check hasn’t even been completed. Of the rest, 2,291 covered cases where initial reviews indicated that the person should have been denied buying a gun. But the government admits that upon further review another 572 of these referrals were found “not [to be] a prohibited person,” leaving about 4,154 cases. That implies an initial false positive rate of roughly 94.2%. And it still doesn’t mean that the government hasn’t made a mistake on the remaining cases. In some cases for example, a person’s criminal record was supposed to be expunged, and it had not been.

Of the cases referred to the BATF field offices there were still a number of false positives. A 2004 sample found out that about **21 percent of these cases** were found to be false positives (the percentage is slightly higher if a weighted sample is used). A discussion of this second round of checks in the **Washington Post** breaks down the 4,732 referrals to the field office in 2010 this way: 10.1% “not a prohibited person,” 8.4% “no potential or unfounded,” 10% “closed by supervisor,” 35% “no prosecutorial merit,” and 23% “guidelines not met.” Adding together the 10.1% “not a prohibited person” and 8.4% “no potential or unfounded” gives a total of 18.5% (which is pretty close to the 21 percent found in 2004) and seems to imply cases where mistakes were clearly made (either because they had the wrong person or they had misidentified something that the person has supposedly done).

Up until this point, no discretion about the merits of the case has entered the picture. If a review of the records indicates that someone is a prohibited individual, they are included. But of these 4,154 cases, only 140 cases involving

banned individuals trying to purchase guns being referred to federal prosecutors, just 60 of which involved providing false information when buying a firearm. Of those 140 cases, federal prosecutors thought the evidence was strong enough to bring a case only 77 times. As the Washington Post evaluation of the 2010 data notes that 35% of the cases show “no prosecutorial merit” these cases aren’t being dropped because of judgments over whether the case will be difficult to win. They are being dropped because there is some mistake in the case.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=1140442

This article argues that both prongs of the conventional wisdom are wrong. Gun control - but only a certain kind of gun control - can be both effective and politically feasible. Research and experience show that gun control works when it reduces the availability of handguns in general circulation. In contrast, the data do not support claims that more modest measures - waiting periods, background checks, enhanced sentences for using a gun in the commission of a crime, and firearm training programs - actually reduce firearm deaths and injuries.

1994 Assault Weapons Ban/ Weapon Bans in general

<https://www.ncjrs.gov/pdffiles1/nij/grants/204431.pdf>

The National Institute of Justice found that if the ban was renewed, the effects on gun violence would likely be small and perhaps too small for reliable

measurement, because rifles in general, including rifles referred to as "assault rifles" or "assault weapons", are rarely used in gun crimes. That study, by the Jerry Lee Center of Criminology, [University of Pennsylvania](#), found no significant evidence that either the assault weapons ban or the ban on magazines holding more than 10 rounds had reduced gun murders. The report found that the share of gun crimes involving assault weapons had declined by 17 to 72 percent in the studied localities. The authors reported that "there has been no discernible reduction in the lethality and injuriousness of gun violence, based on indicators like the percentage of gun crimes resulting in death or the share of gunfire incidents resulting in injury." The report also concluded that it was "premature to make definitive assessments of the ban's impact on gun crime," since millions of assault weapons and large-capacity magazines manufactured prior to the ban had been exempted and would thus be in circulation for years following the ban's implementation

<https://www.nap.edu/read/10881/chapter/6#99>

In summary, the District of Columbia handgun ban yields no conclusive evidence with respect to the impact of such bans on crime and violence. The nature of the intervention—limited to a single city, nonexperimental, and accompanied by other changes that could also affect handgun homicide—make it a weak experimental design. Given the sensitivity of the results to alternative specifications, it is difficult to draw any causal inferences.

A recent evaluation of the short-term effects of the 1994 federal assault weapons ban did not reveal any clear impacts on gun violence outcomes (Koper and Roth, 2001b). Using state-level Uniform Crime Reports data on gun homicides, the authors of this study suggest that the potential impact of the law on gun violence was limited by the continuing availability of assault weapons through the ban's grandfathering provision and the relative rarity with which the banned guns were used in crime before the ban. Indeed, as the authors concede and other critics suggest (e.g., Kleck, 2001), given the nature of the intervention, the maximum potential effect of the ban on gun violence outcomes would be very small and, if there were any observable effects, very difficult to disentangle from chance yearly variation and other state and local gun violence initiatives that took place simultaneously. In a subsequent paper on the effects of the assault weapons ban on gun markets, Koper and Roth (2001a) found that, in the short term, the prices of assault weapons in both primary and legal secondary markets rose substantially at the time of the ban, and this may

have reduced the availability of the assault weapons to criminals. However, this increase in price was short-lived as a surge in assault weapon production in the months prior to the ban and the availability of legal substitutes caused prices to fall back to nearly preban levels. The ban is also weakened by the ease with which legally available guns and magazines can be altered to evade the intent of the ban. The results of these two studies should be interpreted with caution, since any trends observed in the relatively short study time period (24-month follow-up period) are unlikely to predict long-term trends accurately.

In light of the weakness in the theory underlying gun buy-backs, it is not surprising that research evaluations of U.S. efforts have consistently failed to document any link between such programs and reductions in gun violence (Callahan et al., 1994; Police Executive Research Forum, 1996; Rosenfeld, 1996).

<https://www.tandfonline.com/doi/abs/10.1080/13504851.2013.854294>

Using data for the period 1980 to 2009 and controlling for state and year fixed effects, the results of the present study suggest that states with restrictions on the carrying of concealed weapons had higher gun-related murder rates than other states. It was also found that assault weapons bans did not significantly affect murder rates at the state level.

<https://www.propublica.org/article/fact-checking-feinstein-on-the-assault-weapon-s-ban>

The key statistic that Feinstein cited in her recent press release — that the ban "was responsible for a 6.7 percent decrease in total gun murders, holding all other factors equal" — was rejected by researchers a decade ago.

Feinstein attributed the statistic to an initial Department of Justice-funded study of the first few years of the ban, [published in 1997](#).

But one of the authors of that study, Dr. Christopher Koper, a criminologist from George Mason University, told ProPublica that number was just a "tentative conclusion." Koper was also the principal investigator on the 2004 study that, as he put it, "kind of overruled, based on new evidence, what the preliminary report had been in 1997."

Feinstein's spokesman, Tom Mentzer, contested the idea that the 2004 study invalidated the 1997 statistic that Feinstein has continued to cite. But Koper said he and the other researchers in 2004 had not re-done the specific analysis that

resulted in the 6.7 percent estimate because the calculation had been based on an assumption that turned out to be false. In the 1997 study, Koper said, he and the other researchers had assumed that the ban had successfully decreased the use of large-capacity magazines. What they later found was that despite the ban, the use of large-capacity magazines in crime had actually stayed steady or risen.

"The weight of evidence that was gathered and analyzed across the two reports suggested that initial drop in the gun murder rate must have been due to other factors besides the assault weapons ban," Koper said.

<https://www.ncjrs.gov/pdffiles1/173405.pdf>

"The ban has failed to reduce the average number of victims per gun murder incident or multiple gunshot wound victims."

"The public safety benefits of the 1994 ban have not yet been demonstrated."

<https://www.rand.org/research/gun-policy/analysis/ban-assault-weapons/mass-shootings.html>

We identified four qualifying studies that estimated the effects of state assault weapon bans on different aspects of mass shootings. Gius (2015c) found that these bans significantly reduce mass shooting deaths but have uncertain effects on injuries resulting from mass shootings. Using similar models, however, Gius (2018) found that assault weapon bans resulted in significantly fewer casualties (deaths and nonfatal injuries) from school shootings. Using a data set similar to that used in Gius (2015c), Luca, Malhotra, and Poliquin (2016) found uncertain effects of state assault weapon bans on the annual incidence of mass shootings. And Blau, Gorry, and Wade (2016) found that the bans significantly reduced the annual incidence of mass shootings. Considering our assessment of these findings and the relative strengths of these studies, we find *inconclusive evidence for the effect of assault weapon bans on mass shootings*.

<https://www.rand.org/research/gun-policy/analysis/ban-assault-weapons.html>

We found no qualifying studies showing that bans on the sale of assault weapons and high-capacity magazines decreased any of the [eight outcomes we investigated](#).

Critiques of Studies

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=248328

Ian Ayres and John Donohue (1999) provide a useful and positive review of my book "More Guns, Less Crime," and I agree with the directions in which they believe that more work can be done. Yet, there are some serious factual errors in their review and they also never discuss some of the strongest evidence. Finally, I was disappointed that they think that I had not responded to some previous objections raised to my research. Hopefully this response can help explain why these criticisms were mistaken.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=372361

Analyzing county level data for the entire United States from 1977 to 2000, we find annual reductions in murder rates between 1.5 and 2.3 percent for each additional year that a right-to-carry law is in effect. For the first five years that such a law is in effect, the total benefit from reduced crimes usually ranges between about \$2 billion and \$3 billion per year.

Ayres and Donohue have simply misread their own results. Their own most generalized specification that breaks down the impact of the law on a year-by-year basis shows large crime reducing benefits. Virtually none of their claims that their county level hybrid model implies initial significant increases in crime are correct. Overall, the vast majority of their estimates based on data up to 1997 actually demonstrate that right-to-carry laws produce substantial crime reducing benefits. We show that their models also do an extremely poor job of predicting the changes in crime rates after 1997.

Unlike previous authors, Ian Ayres and John Donohue claim to have found significant evidence that right-to-carry laws increased crime. But they have misread their own results. The most detailed results that they report -- follow the change in crime rates on a year-by-year basis before-and-after right-to carry laws are adopted -- clearly show large drops in violent crime that occur immediately after the laws are adopted. Their hybrid results that show a small increase in crime immediately after passage are not statistically significant and are an artifact of fitting a straight line to a curved one. But when one examines a longer period from 1977 to 2000, even this type of result disappears.

A) Is there a "Robbery Effect"?

"Robbery is a good place to start our inquiry because it is committed in public more than other crime, and should be the crime most likely to decline if the Lott and Mustard story of deterrence has any plausibility." (p. 11) "the failure of the

model to show a drop in robbery, casts doubt on the causal story that they advance.” (p. 22) Ayres and Donohue have consistently argued over several papers that robbery is the key result upon which the deterrence by right-to-carry laws is based.⁹ In contrast, Lott has argued many times that there is no a priori reason to believe that the benefits are larger for robbery than other violent crimes.¹⁰ But putting that debate aside, the robbery results presented by Ayres and Donohue present a very clear, consistent story (Figure 2a). The state level analysis shows that robbery rates continued rising, though at a slower rate, for the first two years after the law was passed. However, after that, robbery rates in right-to-carry states fell relative to non-right-to-carry states for the next 9 years, and then remained fairly constant through year 17. The two sets of county level estimates are even more dramatic. Robbery rates in right-to-carry states were rising until the laws were passed and then fell continually after that point. The pattern is very similar to that shown earlier by Lott in examining county level data from 1977 to 1996.¹¹ The changes are also very large. By the time the law has been in effect for six years, the county and state level data imply a drop in robbery rates of 8 and 12 percent respectively. It is difficult to see how anyone could look at these year-by-year results and accept their claims that “robbery effect” is sensitive to the “time frame” examined or to the coding of when state laws were adopted. While Ayres and Donohue acknowledge the problems in using simple before-and-after average in evaluating the impact of the law, yet they do not consistently apply that insight when discussing the evidence.

B) Murder Rates

Figure 2b illustrates Ayres and Donohue’s own year-by-year estimates for murder. Their county and state estimates paint a very consistent picture, but they dismiss the fact that state data estimated a 4.5 percent the drop in murder rates during the first three years of the law as showing “relatively little movement.”¹² Their state level regressions indicate that murder rates were rising in the three years prior to the law being passed and then falling over the next thirteen years. Only one state, Maine, had had the law in effect for more than 13 years. The increase during years 14, 15, 16, and 17 thus solely reflect changes in Maine’s murder rate and since this is state level data each coefficient represents only one data point. The values for these four years show up in the data only because Ayres and Donohue recode Maine’s right-to-carry law as going into effect in 1981 instead of 1985 as previous research had done.¹³ The increase between years 13 and 14 is also more apparent than real. The real “increase” is actually not due to any sudden change in Maine’s crime rates, but due to the fact that other states are included in calculating the crime rate for year 13, while only Maine is used for year 14. Both sets of county level data again imply a large drop in crime that begins immediately after the law has been adopted and continues sharply down after that point.¹⁴ By the time the law has been in effect for six years, Ayres and Donohue’s very own county and state estimates imply that murder rates had fallen by at least 10 percent.

C) Rapes and Aggravated Assaults

Ayres and Donohue's county and state level results for rapes and aggravated assaults are more ambiguous. The county level estimates without the individual state trends show that both rape and aggravated assaults fell almost continually after the laws were enacted (Figures 2c and 2d). Even choosing for comparison the sixth year after the law where rape and aggravated assault rates have slightly risen back up, still leaves rapes 9 percent below their peak and aggravated assaults 3 percent below theirs. The county level estimates with individual state trends provide a mixed picture. With the exception of one single year, rape rates are rising before the law and falling thereafter. In stark contrast using individual state trends changes the aggravated assault rate into a line that rises continuously over almost the entire period until the law has been in effect for 8 or more years. Yet, since the aggravated assault rate was rising for years prior to the law at least as fast as it was after the law it is hard to blame the right-to-carry law for this rise. Ayres and Donohue's state level results are also somewhat ambiguous, though even here the rape rates fall by 10 percent for the first six years after the adoption of the law, and remain below the pre-law levels for at least 12 years. Only when Maine becomes the sole remaining state in the sample does the rape rates rise, and it rises above the pre-law levels for just one year (by 7 percent). Rape rates then plunge by over 25 percent. With only one crime observation present here, the confidence intervals are so large that even with these "wild swings," the changes are too small to conclude that the temporary surge in rapes placed it above the pre-law levels. There is only one year out of the seventeen years after the law has been passed that the rape rate exceeds any of the values during the twelve years before the law. The state level aggravated assault data show only a temporary beneficial effect, with an initial decline in rape that is eventually eliminated. This is similar for aggravated assaults: only three of the seventeen years after the adoption of the law show higher rates than any of the ten years prior to the law.

D) Critiques of Year-by-Year Breakdown of Law's Impact

The debate over simple dummies, splines, or hybrids becomes irrelevant when one has examined the year-by-year breakdown. All those approaches are simple ways to summarize the crime patterns and can provide useful statistics to test whether there is a change in crime rates, but the year-by-year dummies provide a much more accurate picture of changing crime patterns.

Yet, Ayres and Donohue have obviously looked at these estimates from their papers and come to the exact opposite conclusions. Donohue has even taken the year-by-year estimated impact of the law to imply that right-to-carry laws increased crime. In his Brookings paper, Donohue writes (p.20): "For the 1977-97 period [using the results from Donohue's Table 5], the effect for the '2 or 3 years

after' dummy is seen to be highly positive and statistically significant in seven of the nine categories. The other two categories are insignificant, with one negative (murder) and one positive (rape).” Indeed, this is true for his Table 5,¹⁵ but irrelevant. The question isn’t whether these coefficients are different from zero, but whether they have changed relative to other coefficients. The patterns for robbery, murder, and rape clearly show that the longer the law is in effect, the greater the drop in crime.

Nor is it relevant, as Donohue suggests, to compare the crime levels before-and-after the law (p. 20).¹⁶ When crime rates are rising before the law and falling afterwards, there might be very little change in the before-and-after means even though, as the diagram for something like robbery indicates, something dramatic has changed. The key is to compare the trends before-and-after the law, and Ayres and Donohue’s results in Tables 10 and 11 imply large and statistically significant changes.¹⁷

The year-by-year results do not support their claim that “the main effect of the shall-issue laws is positive but over time this effect gets overwhelmed as the linear trend turns down.”¹⁸ Their county level results indicate that by the second and third years after the law has been adopted, all violent crime rates are below the values that they had in the last two years preceding passage of the law.

The figures and the standard errors associated with these estimates also allow us to directly evaluate their claims of model misspecification. One concern is about their claim (regarding the county level data) that “This particular result of a positive main effect and a negative trend effect is inconsistent with any plausible theoretical prediction of the impact of a shall-issue law, since it is not clear why the law should initially accelerate crime and then dampen it.”¹⁹ Yet, their year-by-year estimates shown in our Figures 2a-2d indicate that no such “positive main effect” is occurring.

A claim might be made that the hybrid is mis-specified solely because they are fitting a straight line to a nonlinear relationship. Take Figure 1, where the crime rate is falling at an increasing rate after the adoption of the right-to-carry law. In order to fit a regression with both an intercept shift and a linear trend line to these nonlinear data, the intercept will have to be positive and the trend line will become steeper compared to a specification that uses only a trend line but no intercept shift.²⁰ This does not mean that there is actually an initial increase in crime, but only that it is an artifact of fitting a straight line to nonlinear data.

Table 2 does the same breakdown for all tables listed in Donohue’s Brookings paper, using both the county and state level data for 1977 to 1997 data. While Donohue argues that this evidence strongly shows that concealed handgun laws are not beneficial, all but one of the estimates in his eight tables imply that the costs of crime fall with the passage of right-to-carry laws. The average estimate

implies a saving of \$1.84 billion per year. Simple dummy variable specifications imply much smaller annual gains from right-to-carry laws: they show a gain of \$847 million versus an average benefit of \$2.2 billion for the other specifications. Including a time trend for each individual state reduces the benefits estimated from county level data from \$2.1 billion to \$233 million, though it produces a much smaller reduction in the estimated benefit for state level data. At least for the first five years after the adoption of the law, the spline estimates imply benefits that are almost twice as large as those of the hybrids. The only estimate in Donohue's tables that implies that crime rates would rise uses only a post-passage dummy variable combined with individual state time trends. Returning to Table 1, the losses generated by Ayres and Donohue's Table 13 are dominated by a few states. The table suggests that Kentucky's murder rate increased by an average of 42 percent of the law's first five years, Louisiana's by 34 percent, and Tennessee's by 30 percent. Given that Ayres and Donohue estimated the five-year costs with only one full year of data for Kentucky, Louisiana, and South Carolina, some investigation seems warranted.²⁷ Although a 42 percent increase in Kentucky's murder rate should be easily spotted, this is not the case. Figure 3 shows the actual change in Kentucky's murder rates during the 1990s, and compares it to the change in murder rates for other states in the Midwest and for the United States as a whole. While the US and Midwest murder rates were either unchanged or falling from 1992 to 1995, Kentucky's murder rate was rising. Kentucky's murder rate fell when the law was just getting started in 1996, and continued declining after that. Both percentage declines were much greater than the declines over the same period for the rest of the Midwest or the United States as a whole. Nor do other factors imply that Kentucky should have had an even bigger drop had it not been for the detrimental impact of the law. For example: Kentucky's arrest rate declined by 40 percent between 1995 and 1998 and continued declining after that. A similar breakdown for Louisiana, South Carolina, and Tennessee is available from the authors.

<https://crimeresearch.org/2017/07/badly-flawed-misleading-donohue-aneja-weber-study/>

The bottom line is pretty clear: Since permit holders commit virtually no crimes, right-to-carry laws can't increase violent crime rates. You can't get the 1.5 to 20 percent increases in violent crime rates that a few of their estimates claim with only thousandths of one percent of permit holders committing violent crimes. To put it differently, states would have to miss reporting 99%+ of crimes committed by permit holders for their results to be possible.

The synthetic control tests where they use anything from two to four states to predict the changes in another state's violent crime rates are extremely arbitrary. For example, would you look almost exclusively to Hawaii to predict violent crime rate changes in Idaho, Minnesota, Mississippi, Nebraska, and Utah? Would you look almost exclusively at Illinois to predict changing violent crime rates in South

Carolina? Remember that half of Illinois' violent crime occurs in Chicago and an even larger majority of the changes in Illinois' changing violent crime rate is due to Chicago. Would you look at California and New York to predict changing violent crime rates in Georgia?

There is a reason that the vast majority of published peer-reviewed studies that use US data as this new study does find that right-to-carry laws reduce violent crime rates. [UPDATE: There is a published paper by Carl Moody and Thomas Marvell that points out that even if you use the specifications Donohue, Aneja, and Weber prefer, their results are "fragile and most likely incorrect."]

I. Substance

This new study looks at only murder and violent crime rates, and its most notable claim is that there is some evidence that violent crime rates rose after right-to-carry laws were adopted. But with over 60% of violent crimes involving aggravated assault, any changes in violent crime rates are be driven by changes in aggravated assaults. Other papers in this areas have looked at all the different types of violent crime (such as murder, rape, robbery and aggravated assaults), so it is quite unusual to only examine them as a group and it hides how other crime rates are changing. Donohue's papers are the only ones to claim that there have been increases in aggravated assaults after right-to-carry laws were enacted. We will discuss the problems with this claim below. But more pertinently, permit holders commit virtually no aggravated assaults, especially not aggravated assaults with a weapon.

The authors' purported increase in violent crime is driven by aggravated assaults. No explanation is offered for why having more permits would cause more assaults. No one would suggest that permit holders are more vulnerable to assault, so the claim would have to be that permit holders are doing the assaults themselves. But this is implausible, since permit holders are virtually never convicted of aggravated assaults, let alone aggravated assaults with firearms. Concealed handgun permit holders committing crime can simply not increase either aggravated assaults or violent crime.

Permit holders commit aggravated assaults and violent felonies at rates of thousandths of one percentage point, accounting for hundredths of a percent of the violent crimes or aggravated assaults committed in a state.

On page 45, the authors claim that "official withdrawals clearly underestimate criminality by permit holders," but they offer no evidence for this claim. They provide one case in 2013 from the Huffington Post involving two permit holders who reportedly fatally shot each other. Another case from 2000 is provided, but that permit holder was prosecuted so it isn't clear what this reference demonstrates. The point is clear: Even if convictions of permit holders are somehow being missed by reporting agencies, the error rate would have to be *truly massive* to explain Donohue, Aneja, and Weber's results.

For example, take the discussion below for Michigan where we have data on violent crimes by permit holders. During the 2015-16 year, permit holders might have accounted for 0.053% of violent crime in the state. They claim that Michigan's violent crime rates rose by 8.8% after their right-to-carry law was adopted, that is 166 times larger than permit holder's share of violent crimes. To put it differently, for Donohue, Aneja, and Weber's results to be plausible, *police departments would have to be missing 99.4% of cases where permit holders have committed violent crimes*. For other states the numbers below show similar results: Louisiana police would have to miss 99.5% of crimes committed by permit holders, Oklahoma would have to miss 99.93%, Tennessee 99.98%, and Texas 99.54%.

These percentages assume that no crimes are stopped or deterred by permit holders. To the extent that is true, these percentages would have to be even larger.

In any case, even our numbers overestimate any crimes that might arise from permitted concealed handguns. There are two reasons for this. First, virtually none of even these few violent crimes by permit holders were committed with guns. Second, when you are talking about 600,000 concealed handgun permit holders in Michigan at least a few of them would have committed violent crimes even if there wasn't a right-to-carry law in the state.

Louisiana: Here are the percentages of permit holders who were charged or convicted of any type of felony, whether violent or nonviolent (aggravated assault is one type of felony, but felonies also typically include traffic violations). Including charged cases skews the number substantially, since permit holders have very low conviction rates in general. After all, permit holders are usually arrested even if they used their guns in justifiable self-defense. Police and prosecutors can't just let them off the hook until they are sure about what happened. The vast majority of these cases are unlikely to involve firearms, however. (reports)

2016: 16. Percent of permit holders who are charged or convicted of a felony: 0.0092%

2015: 19. Percent of permit holders who are charged or convicted of a felony: 0.0123%

2014: 15. Percent of permit holders who are charged or convicted of a felony: 0.0109%

Share of violent crimes

2015: There were 25,208 violent crimes in Louisiana, with 19 felony charges or convictions against permit holders. Assuming that these felonies were all violent and that the accused were guilty of the charges, permit holders would account for just 0.08% of the total. Again, this is an overestimate of permit holders' share of violent crimes.

2014: There were 23,983 violent crimes in Louisiana, with 15 felony charges or convictions against permit holders. Assuming that these felonies were all violent and that the accused were guilty of the charges, permit holders would account for just 0.06% of the total. Again, this is an overestimate of permit holders' share of violent crimes.

The authors claim that the violent crime rate in Louisiana rose by 15.4% after the law was their right-to-carry law was adopted. Even if we use all felonies committed by permit holders to estimate their share of violent crimes, for their results to hold, police departments would have to be missing 99.5% of cases where permit holders have committed a violent crime.

Michigan: Below is the percentage of permit holders who were convicted of aggravated assault (with and without a weapon)

2015-2016: 17. Percent of permit holders who are convicted of an aggravated assault: 0.003%

2014-2015: 11. Percent of permit holders who are convicted of an aggravated assault: 0.002%

Also, the percentage of permit holders who were convicted of any type of violent crime (Murder, manslaughter, criminal sexual conduct, armed robbery, unarmed robbery, aggravated assault)

2015-2016: 22. Percent of permit holders who are convicted of a violent crime: 0.00396%

These 22 cases compare to a total of 41,231 violent crimes in Michigan, that is a 0.053% share.

2014-2015: 18. Percent of permit holders who are convicted of a violent crime: 0.00352%

These 18 cases compare to a total of 42,348 violent crimes in Michigan, that is a 0.044% share.

Again, for their results to be plausible, police departments would have to be missing 99.4% of cases where permit holders have committed violent crimes.

Minnesota: Permit revocations due to any type of assault

2015: 0. Percent of permit holders who are convicted of an assault: 0.000%

2014: 0. Percent of permit holders who are convicted of an assault: 0.000%

2013: 0. Percent of permit holders who are convicted of an assault: 0.000%

2012: 0. Percent of permit holders who are convicted of an assault: 0.000%

2011: 0. Percent of permit holders who are convicted of an assault: 0.000%

2010: 0. Percent of permit holders who are convicted of an assault: 0.000%

There were 7,094 aggravated assaults in Minnesota in 2015 and no concealed handgun permit holders were convicted of these crimes. There were also no revocations for other violent crimes. The authors here claimed a -0.7% drop in violent crime after the law was passed.

Oregon: Permit holders who were convicted of any type of felony, violent or nonviolent. The vast majority of these cases are unlikely to involve firearms.

2016: 19. Percent of permit holders who are convicted of a felony: 0.0074%

There were 10,468 violent crimes in Oklahoma in 2015. Even though felonies involve more violent crimes, the 19 felonies that permit holders were convicted of in 2016 equal only 0.182% of violent crimes.

The authors claim that the violent crime rate in Oregon fell by -0.6% after the law was their right-to-carry law was adopted.

Oklahoma: Permit holders who were convicted of any type of felony. (reports)

2016: 20. Percent of permit holders who are convicted of a felony: 0.0071%

2015: 16. Percent of permit holders who are convicted of a felony: 0.0062%

2014: 15. Percent of permit holders who are convicted of a felony: 0.0069%

2013: 15. Percent of permit holders who are convicted of a felony: 0.0078%

2012: 10. Percent of permit holders who are convicted of a felony: Unknown rate because we don't have permit data for 2012.

There were 16,506 violent crimes in Oklahoma in 2015. Even though felonies involve more violent crimes, the 16 felonies that permit holders were convicted of equal only 0.097% of violent crimes.

The authors claim that the violent crime rate in Oklahoma rose by 9.7% after the law was their right-to-carry law was adopted. Even if we use all felonies committed by permit holders to estimate their share of violent crimes, for their results to hold, police departments would have to be missing 99.93% of cases where permit holders have committed a violent crime.

Pennsylvania: We were able to obtain data on revocations by 12 counties for aggravated assaults over the five years from 2012 to 2016. Those counties had an annual rate of 0.0004% of permits revoked for aggravated assaults with a weapon. It is hard to reconcile this tiny number with their claim that violent crimes rose by 26.5% in that state.

Tennessee: Revocations due to any type of assault other than vehicular assault

2016: Zero. Percent of permit holders who are convicted of a non-vehicular assault: 0.000%

2015: Zero. Percent of permit holders who are convicted of a non-vehicular assault: 0.000%

Permit holders who were convicted of any type of felony.

2016: 29. Percent of permit holders who are convicted of a felony: 0.0049%

2015: 31. Percent of permit holders who are convicted of a felony: 0.0061%

There were 40,400 violent crimes in Tennessee in 2015. Even though felonies involve more violent crimes, the 31 felonies that permit holders were convicted of equal only 0.077% of violent crimes.

The authors claim that the violent crime rate in Tennessee rose by 29.5% after the law was their right-to-carry law was adopted. Even if we use all felonies committed by permit holders to estimate their share of violent crimes, for their

results to hold, police departments would have to be missing 99.98% of cases where permit holders have committed a violent crime.

Texas: Convictions for aggravated assault with any type of weapon

2016: 8._Percent of permit holders who are convicted of an aggravated assault: 0.00067%

2015: 10._Percent of permit holders who are convicted of an aggravated assault: 0.0011%

There were 67,727 aggravated assault in Texas in 2015. Even though felonies involve more violent crimes, the 31 felonies that permit holders were convicted of equal only 0.077% of violent crimes.

The authors claim that the violent crime rate in Texas rose by 16.6% after the law was their right-to-carry law was adopted. For their results to hold here, police departments would have to be missing 99.54% of cases where permit holders have committed a violent crime.

Utah: Convictions for aggravated assaults with and without a weapon (permits)

2016: 3 convictions for aggravated assault. 687,382 Permits. Percent of permit holders who are convicted of an aggravated assault: 0.00044%

(1,630

2015: 4 convictions for aggravated assault. 632,276 Permits. Percent of permit holders who are convicted of an aggravated assault: 0.00063%

2014: 4 convictions for aggravated assault. 590,118 Permits. Percent of permit holders who are convicted of an aggravated assault: 0.00068 (all revocations 273 — rate is 0.046%)

2013: 1 convictions for aggravated assault. 535,857 Permits. Percent of permit holders who are convicted of an aggravated assault: 0.00019 (all revocations 320 — rate is 0.0597%)

There were 4,046 aggravated assault in Utah in 2015. Permit holders accounted for only 0.099% of aggravated assaults, though this probably exaggerates their share because two-thirds of Utah's permit holders live outside of Utah where their

aggravated assaults likely occurred. Their paper claims that Utah's violent crime rate fell by 20.2%.

IX. Synthetic approach

In the synthetic approach, that Donohue, Aneja, and Weber use they use from 2 to 4 states to predict changes in crime rates for right-to-carry states. But to say that it is arbitrary what states that they end up using for comparisons is an understatement. For example, if you wanted to predict how Georgia's violent crime rate would change over time, would your first choice be California and New York? If you wanted to predict the changing violent crime rates in states as diverse as Colorado, Idaho, Minnesota, Mississippi, Nebraska, and Utah, would you primarily or almost exclusively rely on Hawaii? Similarly, Louisiana and Tennessee might be surprised that these authors think that you should rely on Chicago (where half of Illinois' violent crimes occur and the vast majority of changes in Illinois' violent crime rate arise).

To put it mildly, their results are crucially dependent on what states that they pick to compare, and they have a lot of control over which states that they pick. The issue isn't really whether they use rules that result in Hawaii or some other state to make the comparison, but the fact that how they pick the states and the number of states that they use to compare is arbitrary.

X. Lumping all violent crimes together

Other papers in this areas have looked at all the different types of violent crime (such as murder, rape, robbery and aggravated assaults), so it is quite unusual to only examine them as a group and it hides how other crime rates are changing. Let's assume for a second that aggravated assaults. Crimes involving aggravated assaults are on average much less harmful to people than the other violent crimes: murder, rape and robbery. It is simply not honest to lump all these crimes together as it would hide the true changing cost of violent crime.

XI. Conclusion

There are many other points that have been previously raised and could be raised again, but these are three major mistakes in the Donohue, Aneja, and Weber paper. But the media doesn't even try to get both sides of the story, eschewing the most fundamental tenet of journalism. The authors also commit intellectual malpractice by refusing to address longstanding objections to their methods, which create the false impression that right-to-carry laws increase violent crime.

<https://crimeresearch.org/2017/07/responding-john-donohues-responses-evaluati-on-new-study/>

Mr. Yablon then posted Donohue's responses to our critique of the John Donohue, Abhay Aneja, and Kyle Weber's paper.

Lott says: "No other study by an economist, criminologist, or law professor has claimed that US violent crime rose after right-to-carry laws were adopted."

But Lott has criticized his coauthor (Zimmerman) who says exactly that in a 2014 paper, which is described in our paper as follows:

Zimmerman (2014) examines the impact of various crime prevention measures on crime using a state panel data set from 1999-2010. He finds that RTC laws increased murder by 15.5 percent for the eight states that adopted RTC laws over the period he analyzed.... Zimmerman describes his finding as follows: "The shall-issue coefficient takes a positive sign in all regressions save for the rape model and is statistically significant in the murder, robbery, assault, burglary, and larceny models. These latter findings may imply that the passage of shall-issue laws increases the propensity for crime, as some recent research (e.g., Aneja, Donohue, & Zhang, 2012) has suggested".

Moreover, our paper also notes that "Durlauf, Navarro, and Rivers (2016) attempts to sort out the different specification choices in evaluating RTC laws by using a Bayesian model averaging approach using county data from 1979-2000. Applying this technique, the authors find that in their preferred spline (trend) model, RTC laws elevate violent crime in the three years after RTC adoption: "As a result of the law being introduced, violent crime increases in the first year and continues to increase afterwards"."

What Donohue leaves out is that the sentence after the end of the sentence he quotes says: **"However, as the shall-issue law impact is being identified from only eight state changes in the data, it is difficult to give any strong causal interpretation to these estimates."** More importantly, the whole point of Zimmerman's paper is to use an instrument and to show that without that instrument you can get biased results. In Table 4 in Zimmerman's paper, there is **no statistically significant result for the right-to-carry law variable**. As has been pointed out before by John Lott, there is a good reason to believe that **estimates that look at only later years and do not account for differences in right-to-carry laws are biased towards not finding a benefit**.

Our question to John Donohue is why didn't you include the next sentence in your quote? Nor does Donohue reference a later paper by Zimmerman and some co-authors that found: **"the evidence shows that RTC laws are socially beneficial."**

Donohue also very selectively quotes the Durlauf, Navarro, and Rivers. The end of the first paragraph in their conclusion is pretty clear: **"Overall, we conclude that the evidence that shall-issue right-to-carry laws generate either an increase or decrease in crime on average seems weak."**

Lott also tries to argue that permit holders are so law abiding that they could not commit enough added crime to result in major violent crime increases, but this point is addressed at length in the paper. Some very bad things are done by permit holders, as indicated by two stories in just the last few days:

- 1) A Pennsylvania man with a concealed carry permit kills an 18-year-old college bound girl in a road rage incident. <http://www.delcotimes.com/article/DC/20170703/NEWS/170709926>
- 2) An intoxicated man with a concealed carry permit in Seattle gets into an argument with his wife while taking an Uber home from a wedding and fatally shoots her in the head. <http://www.seattlepi.com/local/crime/article/Charge-Drunk-gun-wielding-husband-killed-wife-11270993.php>

But permit holders also help others commit crime because carrying guns outside the home greatly increases the chance of theft (and loss). Michael Rallings, the top law enforcement official in Memphis, Tennessee, noted the problem of leaving guns in motor vehicles, which greatly facilitates theft: **“Laws have unintended consequences,”** according to Rallings. **“We cannot ignore that as a legislature passes laws that make guns more accessible to criminals, that has a direct effect on our violent crime rate.”** My rough sense is that for every time a gun is used defensively to address violent crime, 10 guns are stolen, thereby arming criminals. <https://www.thetrace.org/2017/03/as-thefts-of-guns-from-cars-surge-police-urge-residents-to-leave-their-weapons-at-home/>

Moreover, as we have seen with Philando Castille and other incidents, gun carrying creates problems for police. In a sense RTC laws are a tax on the most effective element of crime reduction – well trained police, and therefore crime overall is stimulated.

In our previous discussions of Donohue’s claims, we provided **systematic data sources** from state police bureaus in eight states. We also previously noted the two news stories that Donohue provided. Here he provides two more cases from the last couple of weeks. This is a very strange debating technique in that no one is claiming that the 16 million permit holders in the US never commit crimes. The point is that they very rarely commit any crime, and very rarely commit firearm related offenses.

Lott tries to claim that our synthetic control results are flawed because Hawaii was not a good control for various other states. That might be a reasonable criticism, except the paper addresses it by dropping each synthetic control state and re-evaluating the estimated impact on RTC laws. The aggregate estimates were extremely robust and in fact when Hawaii was dropped, the estimated increase in violent crime rose somewhat. See Figure 11 of the paper.

In another paper (<https://crimeresearch.org/2017/07/badly-flawed-misleading-donohue-aneja-weber-study/>) Lott states: "To put it mildly, their results are crucially dependent on what states that they pick to compare, and they have a lot of control over which states that they pick."

But this is totally incorrect. The synthetic controls approach was designed to eliminate the ability of the researcher to choose among the specified control states. It is done by a maximization program. This is made clear even in the abstract of the Abadie paper that created this statistical tool.

Donohue's response is simply not honest here. There is a great deal of arbitrariness in terms how the synthetic approach is set up. For example, is the window that they should have used to compare states 3 years, 5 years, 8 years, 10 years, 15 years or some other length? By saying that you will only create a synthetic using states that have not adopted a right-to-carry law for at least 10 years after the target state has adopted the law, you are throwing out a lot of comparison states and a lot of information. There is no theory that says what the right window is. The choice of control variables has a big effect on what states will be used to create the synthetic state. Donohue has a reputation for only reporting a very tiny percent of all the regressions that he runs. If you are willing to run thousands of regressions, you can end up finding some set of states that are used to create the synthetic states that will give you any result that you want.

Lott alludes to earlier articles using panel data analyses (most done by himself or his supporters) that have found different results but he doesn't mention that these papers were done on far fewer years and right to carry adopting states than our paper. Indeed, we run the exact Lott panel data model on this longer data set and show the results that

Lott says our his most preferred – which is the spline model of Table 6 Panel A in our paper. Our table shows what the Lott model predicts: each year the murder rate increases following adoption of RTC laws (rising by 6.5 percent after 10 years). These results are statistically significant.

Donohue ignores the concerns that have been raised for over a decade about the changing type of states that adopt right-to-carry laws. Later adopting states have much more restrictive regulations in obtaining a concealed handgun permit. For example, Illinois, the last state to adopt a right-to-carry law, requires that you have to get 16 hours of training and the fee is \$150. The earliest states had no training requirements and very low fees. If you include these later years (even worse if you look at years from 2000 to 2014), you are comparing restrictive right-to-carry states that changed their laws during the sample period to earlier states that have extremely liberal right-to-carry laws. Lott has shown that this problem can actually reverse how you interpret the results from these regressions.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3246449

John Donohue and colleagues recently assessed the impact of "right-to-carry" (RTC) laws on crime rates. These laws make it easier to get a carry permit. Donohue et al. claim that their analysis indicates that, contrary to what nearly all other researchers have found, these laws increase violent crime. This paper presents a critical analysis of the research by Donohue et al., and shows that their conclusions are unwarranted.

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3084736

Daniel Webster and his colleagues (2014) tried to estimate the effect of Missouri (MO) repealing its handgun permit-to-purchase (PTP) law in 2007, using an unsuitable research design, misspecified statistical models, and a biased sample of states. They also examined simple trends in homicide rates in MO and in comparison states. They concluded that the repeal caused an immediate (and astounding) increase of 25% in the firearm homicide rate. They attributed this result at least partially to increased “illegal gun diversion” (a term they never defined). Their results cannot be relied upon to indicate whether the repeal actually caused any homicide increase, or increased movement of guns into criminals’ possession. This comment explains why

In sum, Webster et al. had no scientific foundation for drawing even the weakest conclusions about trends in gun trafficking or “illegal gun diversion” after MO repealed its PTP law.

The point is not that we can tell anything useful about the reasons for homicide changes in either Iowa or Nebraska or MO from these kinds of simplistic comparisons. Rather, the Iowa and Nebraska data demonstrate that single states can easily experience year-to-year homicide increases just as large as that observed in MO without it being due to the repeal of a PTP law or any other gun control measure, that it could happen at the roughly the same time as it happened in MO, and in the same part of the country. Thus, this simple comparison does definitively establish the simple point that MO’s homicide increase could easily be entirely due to other factors, like those operating in neighboring Iowa or Nebraska, besides the repeal of a PTP law

It is almost as if Webster et al. were picking and choosing control variables on some basis other than one grounded in their own empirical evidence, theory, or prior research. This is especially worrisome, because it is possible to manipulate the estimated effect of a given variable simply by failing to control for confounders. Confounders are, by definition, variables whose control will affect estimates of the variable with which they are associated. That is, failing to control for a genuine confounder will distort the estimate of the variable with which the confounder is correlated. For example, Kleck (2017) reanalyzed the data underlying a study in which the authors had found a large significant positive association between gun rates and suicide rates (Miller et al. 2007), and showed that when five genuine confounders were controlled that had not been controlled in the original analysis, the association initially observed between guns and

suicide disappeared. The original analysis had only controlled for, at most, a single genuine confounder. In the conclusions to their report, Webster et al. give the impression that they had ruled out a substantial number of plausible alternative explanation of the MO post-repeal homicide increase, listing no less than eight variables or categories of variables that could not explain this increase. This listing is deceptive because few of these implied alternative explanations were plausible in the first place, so ruling them out was a largely pointless exercise. Further, of the eight, they could in fact rule out just three of them. None of their analyses rule out the possibility that there was an outbreak of homicide in MO in 2008-2010 that was entirely caused by factors other than the PTP repeal. The other factors that Webster et al. claimed to have ruled out would not be considered by knowledgeable scholars to be likely alternative explanations of this short-term homicide increase anyway, either because the variables do not in general affect homicide rates (e.g., unemployment rates, as officially measured; policing levels; MO's Stand Your Ground Law) or because they do not change enough over short periods of time to cause large short-term homicide increases (incarceration rates).

Note that the authors vaguely allude to use of UCR data for 1999-2012, but this was done to add two years to the time series (p. 295), a relatively trivial improvement. There is no indication that they used UCR data so as to cover the entire U.S. and eliminate the severe sample bias in their dataset based on vital statistics age-adjusted homicide data.

Thus, Webster et al.'s conclusion ultimately stands or falls on the basis of the size of a single data point, the homicide rate in MO in 2008. If they were wrong about why homicide was higher in MO in 2008, their entire case for a homicide-elevating effect of the PTP repeal collapses. Thus, their conclusion rests on an extremely fragile foundation. We have shown that they have no reliable foundation for their claim that the repeal caused an increase in gun trafficking, illegal gun diversion, or gun possession among criminals, since they was not able to measure any of these things. Consequently, they had no basis for claiming that the repeal put more guns in criminal hands, regardless of the mechanism by which this might have occurred. We have also shown that Webster et al. ruled out only a single alternative explanation of the post-repeal homicide increase (changes in poverty) in his analyses of the less biased UCR-based sample, so they had no sound basis for seizing on the PTP repeal as being responsible for the 2008 jump in MO homicide.

As far as Webster et al. can demonstrate, it is just coincidence that this increase happened to follow the PTP repeal. There is also nothing special about a homicide increase this large, since even larger ones occurred in Nebraska and

Iowa. And there is nothing special about it occurring around 2008, as the Iowa and Nebraska homicide increases occurred in the same period, even though neither state repealed a PTP law. Webster and his colleagues insist that there is something significant about the fact that this large homicide increase occurred specifically in the firearm homicide category. They appear to be unaware that, when homicide in general is increasing, for whatever reason, gun homicide always shows proportionally larger increases than nongun homicide. Even when gun law is unchanged and gun ownership levels are stable, one can still routinely expect changes (upward or downward) to be proportionally larger in the gun homicide category than in the nongun homicide category (Britt, Kleck, and Bordua 1996).

Second, Webster et al. did not include the lagged homicide rate as a predictor of the current homicide rate. This procedure, in this instance, would control for homicide in the previous year to estimate the effect of the other variables on homicide in the current year. It serves to indirectly control for variables not explicitly measured that may nonetheless affect homicide rates, and thus better separate out the effect of the PTP repeal. Thus, an analyst could control for other factors that might have influenced the MO homicide trends in 2008-2010 besides the repeal of the PTP law without actually measuring them, based on the simple assumption that factors that affected the homicide rate before 2008 probably continued to affect them after 2008. The lagged homicide rate, then, serves as a proxy for these unknown or unmeasured confounding variables.

The approach used by these authors is useless for assessing the impact of changes in gun laws, but can easily be used to generate results that appear to support the researchers' policy preferences, whatever they might be, and regardless of the actual effects of gun law changes. Thus, I suspect that we have not seen the last of this methodology

<https://www.hoplophobia.info/wp-content/uploads/2015/10/Gun-Crime-Methodological-Review-of-the-Evidence.pdf>

Purpose: This paper reviews 41 English-language studies that tested the hypothesis that higher gun prevalence levels cause higher crime rates, especially higher homicide rates.

Methods: Each study was assessed as to whether it solved or reduced each of three critical methodological problems: (1) whether a validated measure of gun prevalence was used, (2) whether the authors controlled for more than a handful of possible confounding variables, and (3) whether the researchers used suitable causal order procedures to deal with the possibility of crime rates affecting gun rates, instead of the reverse.

Results: It was found that most studies did not solve any of these problems, and that research that did a better job of addressing these problems was less likely to support the more-guns-cause-more crime hypothesis. Indeed, none of the studies that solved all three problems supported the hypothesis.

Conclusions: Technically weak research mostly supports the hypothesis, while strong research does not. It must be tentatively concluded that higher gun ownership rates do not cause higher crime rates, including homicide rates.

<https://poseidon01.ssrn.com/delivery.php?ID=274113064065074070092070125075080107105033003077055038110064084100121113005104011071001030031003012032002100106031025127021030039038064079079115014080004114111007019048085102116011065086124010088096113070021094125025121081099098097124007070107082092&EXT=pdf>

Webster, McCourt, Crifasi, Booty, and Stuart (2020) recently published an article in which they concluded, based on a panel study of annual state-level data, that the incidence of mass shooting incidents and the total number of fatalities linked with such incidents are reduced by two types of gun control law: bans on large-capacity magazines (LCM) and purchasing licensing laws that require applicants to personally appear at a public safety agency or that require them to be fingerprinted. This comment establishes that the finding regarding the effect of LCM bans is inconsistent with the authors' own technically soundest findings, and that the finding regarding purchaser licensing is highly sensitive to variations in method, and may have been produced by data-dredging.

Laws banning LCMs cannot affect the incidence of MS or the number of people killed in MS unless LCM use somehow affects these outcomes. The authors, however, never explain why or how LCM possession or use would affect either whether a prospective mass shooter would carry out a MS or the number of people killed. For example, on p. 188 they insist that "there is a clear functional link between LCMs and the ability of a shooter to take more lives," but do not provide even a hint as how this link might work. I am not aware of either advocates or scholars who have argued that any people otherwise inclined to commit a MS refrain from doing so because they would have to use, e.g., three 10-round magazines rather than a single 30-round magazine. Instead the standard arguments in favor of LCM bans is that, by denying LCMs to prospective mass shooters, and forcing them to use smaller capacity magazines, they would have to reload more often. This supposedly would increase opportunities for bystanders to stop the shooter or increase the time when potential victims could escape or hide. This line of reasoning, however, has been shown to be inconsistent with the way MS are actually carried out, since (1) known cases of bystanders intervening during a mass shooter's attempt to reload are virtually nonexistent (it's possible none have occurred in the past 30 years), and (2) 3-4

second reloads of detachable magazines do not increase the time when shooters are not firing (Kleck 2016). The authors do not propose any alternative explanation of why LCM use would affect “the ability of a shooter to take many lives,” or refute any of the evidence or logic presented in Kleck (2016), so they have no logical basis for interpreting an association between MS and LCM bans as indicative of a causal effect of the latter on the former.

The results are also inconsistent with expectations regarding which kinds of MS were supposedly affected by LCM bans. Domestic violence-linked MS typically occur in the shooter’s (and victims’) home, where the shooter could have easy access to as many guns and magazines as he wanted, making LCMs unnecessary for shooting many people. In contrast, MS committed against victims who are strangers to the shooter typically occur in public places where a shooter would be more limited in terms of how many guns and magazines he could easily carry without detection. MS linked with domestic violence also have fewer victims per incident, making LCM use less necessary to harming as many victims as the shooter wishes to hurt – you don’t need an LCM to shoot 4 or 5 family members in your own home. Consequently, if LCM bans had any causal effect on MS, one would expect the effects to apply to “Non-Domestic Linked Fatal Mass Shooting Incidents” but not “Domestic-Linked Fatal Mass Shooting Incidents.” The authors obtained precisely the opposite pattern of findings – a significant negative association of LCM bans with domestic MS (Table A6) but no significant association with non-domestic MS

Taken as a whole these patterns of findings strongly suggest that the statistical associations between LCM bans and MS that the authors obtained were spurious, i.e. not reflective of actual causal effects of LCM bans. Such noncausal associations could be produced by a failure to control confounding variables, discussed next. The authors also failed to do the most direct test of the effect of an LCM ban. If such a law had any actual causal effect on MS, it should have had the effect specifically on those MS that involved use of LCMs. Even though the data are available to do so, the authors did not separately measure LCM-involved MS, and thus did not test whether it was specifically those kinds of MS that were lower in state-years with LCM laws. Or at least they do not report doing this obvious test. A cynical reader might suspect that they did perform this test but found that it was not LCM-involved MS that were lower in state-years with LCM bans, casting serious doubt on the view that the lower level of MS in those state-years reflected a causal effect of LCM bans.

Data-dredging can take many forms, but in connection with the authors’ findings pertaining to “Purchaser licensing in-person application/fingerprinting required” it could have taken the form of redefining the favored form of gun control in

numerous ways until one happened to correlate with fewer MS or MS fatalities. The definition they ending up using seems like an oddly narrow, specific category of gun control, just one of many variants they might have assessed. For example, the authors did not assess the impact of all laws requiring a background check to buy a gun, nor did they assess all laws requiring a purchase permit. Instead they ending up testing the supposed effects of a very specific subset of those laws, out of the many variants they could have assessed. Instead they assessed a variant of purchaser restrictions that either (1) required the applicant to appear in person at a public safety agency, or (2) required the applicant to be fingerprinted. Note that, in order for a state's law to qualify, it was not necessary that the law require that applicants to personally appear at a public safety agency and submit to fingerprinting; it was only necessary that the law require either of these two. Coding a state-year as having such a law was thereby broadened to include a different, larger set of state-years. If this broader set of state-years happened to have fewer MS, even if for reasons other than a causal effect of the purchaser licensing law, the results would give the impression that the law caused less MS. Likewise, the authors chose to define the purchaser licensing law as one applying to handguns, even though they claimed that many MS are committed using assault rifles. They could have defined the variable to cover laws that applied to purchases of all guns, including rifles and shotguns, but did not, even though such a definition has obvious merits. This is yet another seemingly arbitrary definitional decision made by the authors that could generate different results because different sets of state-years would have been defined as subject to the "purchaser licensing" law as defined by Webster and his colleagues. In sum, they may have simply tried out numerous different versions of the purchaser licensing law until they finally used a definition such that state-years possessing such a law happened, by chance, to have fewer mass shootings. This sort of data-dredging may also have generated the similar findings that Webster obtained in eight prior studies in which he was a co-author (all very similar to the present study) and which he boasts were "rigorous" (cited p. 172). Webster and his colleagues appear to think that the consistency of findings regarding purchaser licensing confirms their view that such laws really do reduce violence, but of course the same kind of consistency could be obtained by indulging in the same sort of data-dredging in all of the studies. The authors' definition of purchaser licensing laws would look less arbitrary and less likely to have been the product of data-dredging if the authors had provided evidence, or at least some convincing argumentation, as to why the specific variants of purchaser licensing laws they stressed would affect mass shooters to a greater extent than other limits on gun purchases. They attempt to do so on p. 172 but their arguments make no sense. For example, they argue that background checks under the variant of purchase restrictions that they favor are more thorough because they provide "greater time available to conduct those checks," but do not explain what additional checks are done under these laws that are not also done under the computerized "instant check" provisions mandated by the Brady Law or similar state laws. Nor do they cite any evidence that background

checkers actually use the “greater time available” to consult paper records or any other sources of information not consulted under computerized instant background checks. The authors likewise hint that fingerprinting of applicants somehow facilitates checks of “more complete records of prohibiting incidents” (p. 172), without explaining how or why fingerprinting provides access to records of “prohibiting incidents” that are not covered by computerized background checks conducted without fingerprinting, and without citing any empirical evidence that fingerprint-assisted background checks actually do uncover more “prohibiting incidents.” The authors also note that some purchaser licensing laws require gun safety training. This would seem to be totally irrelevant to MS, unless they were seriously suggesting that gun safety training could prevent MS, as if such shootings were accidents or the result of a lack of knowledge of how guns work. Another interpretation, however, is that the authors were straining to justify the seemingly eccentric and narrow way they defined restrictions on gun purchasing, and thereby make it seem less arbitrary. In any case, they did not require that a state’s law require safety training in order to qualify as possessing their target law, so it is hard to see the relevance of such training.

The authors repeatedly obtained results indicating that gun ownership rates have no significant effect (and, in some models, a nonsignificant negative association at that) with mass shootings and MS fatalities (Tables 3-5, A2-A15). One might think that this is major news and would be a key finding emphasized in the Research Summary and the Conclusions. Indeed one might even expect a title for the article such as “Gun Ownership Rates Have No Effect on Mass Shootings.” The authors, however, chose to ignore these findings – they are not mentioned in either the Research Summary or the Conclusions section.

<https://crimeresearch.org/2018/03/problems-siegel-et-al-american-journal-public-health-easiness-legal-access-concealed-firearm-permits-homicide-rates-united-states/>

Study: <https://pubmed.ncbi.nlm.nih.gov/29048964/>

Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States

Michael Siegel ¹, Ziming Xuan ¹, Craig S Ross ¹, Sandro Galea ¹, Bindu Kalesan ¹, Eric Fleegler ¹, Kristin A Goss ¹

Siegel et al cut falsely claim: "In 2 studies, shall-issue laws were found to decrease homicide rates. In 2 studies, these laws were found to increase homicide rates."

But 8 other published papers use state-level data and find that concealed carry laws reduce homicide (other studies find this benefit looking at county or city-level data).²⁻⁹ And, in fact, neither of the papers that they cite to show that concealed carry laws increase murder actually find the increase they claim.

Zimmerman's paper provides two sets of estimates.¹⁰ While the first set does show statistically significant results, Zimmerman warns, "However, as the shall-issue law impact is being identified from only eight state changes in the data, it is difficult to give any strong causal interpretation to these estimates." When he provides his preferred instrumental estimates, which adjust for certain biases, Zimmerman finds no statistically significant results.

As to Ludwig's results, they were never statistically significant.¹¹ In discussing Table 4, he writes: "The standard errors imply that the point estimate is not statistically significant." None of the results in Table 5 are statistically significant at the 10 percent level for a two-tailed test.

<https://crimeresearch.org/2013/12/problems-with-public-health-research-michael-siegel-craig-ross-and-charles-king-the-relationship-between-gun-ownership-and-firearm-homicide-rates-in-the-united-states-1981-2010-ajph/>

Study: <https://pubmed.ncbi.nlm.nih.gov/24028252/>

Arguments?

Tyranny, most simple semantical point to win on but use this if all else fails

“Why do you need it (“it” being contextual, it can be semi-auto, multiple rounds, full auto, or even guns in general)

Answer(s): Self defence from those who would aggress upon you. Resistance to government tyranny. Hunting (even though this is a weak point). Train yourself on semantics for this point as it is really easy to argue but also really easy to corner yourself.

“You really think you would be able to fight against the military with guns?”

Answer(s): Yes. (This question actually helps you as it proves your point that tyranny is a very real threat, also this is the argument for legalization of nukes.)

I will elaborate: Even if one concedes that the military would always beat the citizens if they were to revolt, the opposition made the argument that we should just sit and watch as a tyrannical state takes away your rights and possibly kills more people, which is also a utilitarian argument against gun control (I will explain later).

This point helps you make the argument for nukes, because if the opposition says that the military would just nuke you, (which they would not as the government would not destroy its own land, which a state will always attempt to get more of, as it is a side effect of the state, *read Anatomy of The State*) this instantly gives you the ability to say

“So give us the weapons the government has”

If you say this they will most likely not have a response But if they do, it would be very weak.

This should be enough to carry you against your average radlib

Rights (must read)

<http://www.owl232.net/papers/guncontrol.htm>

Missouri 2014 study

<https://crimeresearch.org/2014/02/cprc-at-fox-news-media-cherry-picks-missouri-gun-data-to-make-misleading-case-for-more-control/>

<https://crimeresearch.org/2014/02/what-does-missouri-show-about-the-benefits-from-universal-background-checks-the-forthcoming-journal-of-urban-health-study-by-the-bloomberg-school-of-public-health/>

https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3084736

Refutations to Studies finding that Concealed carry laws increase crime

Study list:

https://www.researchgate.net/publication/307622658_Prevention_of_firearm-related_injuries_with_restrictive_licensing_and_concealed_carry_laws_An_Eastern_Association_for_the_Surgery_of_Trauma_systematic_review

Mcdowall study refutation

<https://scholarlycommons.law.northwestern.edu/cgi/viewcontent.cgi?article=6858&context=jclc>

